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PERSONALITY TRAITS BY FACTORIAL ANALYSIS (I).

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PERHAPS the best statement of the nature of personality traits is that given by Betts.[1] "A trait is a habit or closely knit habit system of sufficient stability and importance to receive a name, such as dependability, sincerity, etc. Traits can never be completely generalised. Yet for well integrated personalities traits do come to possess sufficient coherence and reliability so that behaviour reactions are reasonably predictable."

Personality "testing" has revealed comparatively consistent patterns of social habits—these may be called "traits". G. W. Allport has stood consistently for the theory of generalised traits in this sense. Gilliland, too, agrees that there are, ordinarily, enough common factors running through an individual's conduct to warrant the postulation of unique traits. He has analysed personality into five tentative major traits which he regards as fairly independent—(1) Intelligence, (2) Aggressiveness, (3) Sociability, (4) Personal appearance and (5) Morality. Allport, Bernreuter and many others have similarly determined "traits" in this "a priori" fashion. To use Lorge's very descriptive phrase they have created traits "by fiat". Such creation of traits by decree is unsatisfactory from two points of view. It does not define the trait in any consistent empirical way, and such a decreed trait may be found to be not consistent or homogeneous. Secondly, it does not guarantee independence of traits and unless there is this independence traits overlap and more than one concept

[1] G. H. Betts: "The Foundations of Character and Personality", Bobs-Merrill, 1937.

is used to explain a single behaviour tendency. Traits, ideally, will be homogeneous, consistent and independent.

When one thinks of traits as "patterns of social reactions" it is obvious that absolute consistency will not be achieved. Personality is a function of the person and the social situation. Reactions vary as the situation varies. Especially is this the case with children. One would not expect, therefore, to find traits so well defined among a juvenile group as among adults. On the other hand observation and test experience show that social behaviour is not wholly specific.

The isolation of unique or independent traits may be made statistically. By sampling social situations widely it is possible to obtain a number of determinations of the individual's behaviour. Experience has shown that certain reliable patterns can be isolated from this complicated set of data. The statistical technique of factorial analysis is designed to make this isolation possible. By definition, then, the application of the factorial technique to data derived from carefully prepared questionnaires and tests should yield unique traits of personality.

The traits thus established will be defined much as Betts has defined them. They will be closely knit systems of social habits of sufficient stability and importance to receive a name; and of sufficient coherence and reliability to make their measurement possible and predictions from them worth while.

THE LEGITIMATE USE OF STATISTICAL METHOD.

A growing group of studies have utilised the multiple factor methods of analysis of personality test material. Factorial analysis may be regarded as an effort to isolate the elements of this material.

Terman[2] has warned that though factorial analysis has demonstrated its utility in the analysis of personality tests, that though it is well to know that a vast array of alleged personality "traits" may possess altogether not more than four or five distinguishable components, it is another thing to

[2] L. M. Terman: "The Measurement of Personality", *Science*, 1934, 8, pp. 605-8.

conclude that personality can be accounted for so simply. There must be many factors which cannot be embodied in our tests, and this will continue to upset prediction from the test scores.

The limitation is admitted. On the other hand the case for the continued use of the factorial technique is stated by Shaffer.[3] "The method of factor-analysis, though impossible to explain in non-technical terms and laborious to use, probably holds the key to progress in personality measurement. It shows that the proper procedure is to discover the existence of independent traits and to explore their nature; then to name them afterward. Only by rigorous mathematical methods such as this can the real elements of human personality be distinguished with precision."

The legitimate use of statistical method is as an analytic device only. Subsequent synthesis will be the work not of rigorous mathematical formulæ but of psychological intuition. "The point is sometimes stressed by critics of the psychometric approach that although this can reveal the existence of factors it cannot identify them, intuitive judgment having ultimately to be called in at this point. . . ."[4] The intuitive transformation from the mathematical to a psychological interpretation of the analyses has usually been the focal point of criticism of the factorial approach.

EARLY STATISTICAL ANALYSIS.

Spearman in 1904 seems to have been the first to apply a rigorous analytic technique to test data. The pioneer investigation in the field of personality, however, was that of Webb[5] in 1915.

WEBB'S ANALYSIS OF RATINGS.

Webb's primary concern was "an exact study of character". He tested for intelligence 190 training college

[3] L. F. Shaffer: "The Psychology of Adjustment", Houghton-Mifflin, 1936, p. 300.

[4] F. C. Thomas: "Intuition or Psychometry in the Study of Personality", *Char. and Pers.*, 1939, 7, pp. 309-317.

[5] E. Webb: "Character and Intelligence", *Br. J. Psych. Monog. Supp.* 1, No. 3, 1915.

students and 140 schoolboys. Each of these subjects was then assessed by class prefects and school teachers for a number of intellectual, moral and social traits. Among these traits were such as conscientiousness, impulsive kindness, readiness to become angry, tact and so on. Using partial correlation Webb eliminated the effect of general intelligence. Applying then Spearman's analytic technique he found a set of significant correlations between eight particular traits. From these he isolated a factor which he called "w", and which he believed to represent general strength of will. It was strongly loaded with traits such as conscientiousness, trustworthiness, kindness on principle and persistence. This new factor, a character factor as distinct from the general intellectual factor "g", he concluded was closely related to "persistence of motives". It is not a cognitive but a conative factor.

Vernon's criticism[6] of Webb's work is pertinent. "Webb could not have been aware at that time of the pervasiveness of halo; but it is clear now that the traits listed are precisely of the desirable kind which his raters would be most likely to attribute to those whom they regarded through a favourable halo. How far then 'w' should be considered as a fundamental dimension of the ratees' personalities, and how far it corresponds to their mere popularity, cannot be decided."

BURT'S INVESTIGATION OF EMOTIONAL TRAITS.

In the same year (1915) Burt published a report entitled "General and Specific Factors Underlying the Primary Emotions"[7] (*Rep. Brit. Ass.*, p. 694). He studied the intercorrelations between assessments upon the primary emotional traits suggested by McDougall, in his "Social Psychology". The traits selected were—anger, assertiveness, sociability, curiosity, joy, sex, disgust, tenderness, sorrow, fear and submissiveness. He used not only ratings but also records of observed behaviour which were classified under the various emotional traits. While such classification does not achieve

[6] P. E. Vernon: "The Assessment of Psychological Qualities by Verbal Methods", H.M. Stationery Off., London, 1938, p. 62.

[7] Position re-stated, C. Burt: "Analysis of Temperament", *Brit. J. Med. Psych.*, 1938, 17, pp. 158-188.

much objectivity it should have dampened the effect of halo.

In the later (1938) report of this research advanced statistical techniques have been employed and the result has been to reveal three personality or emotional factors. In the words of Burt's own summary (p. 187), "Assessments have been obtained from the behaviour of 124 delinquent and neurotic children, and covariances and correlations calculated between the primary emotional traits. Four factors appear sufficient to account for the correlations, and two for 90% of the variance.

"Apart from the factor of general emotionality, the most important are two bipolar factors making (i) for aggressive as contrasted with inhibitive emotions, and (ii) for pleasurable as contrasted with unpleasurable emotions respectively."

Elsewhere[8] Burt compares his factor "e" or general emotionality with Webb's factor "w". Both, he points out, are conative factors; both enter into voluntary behaviour; and variations in both constitute elements of character. There are, however, differences in that Burt has taken account of alleged innate traits only, while Webb's data included much that was acquired. The major difference between "w" and "e" may be summed up by saying that "e" is the inverse of "w". The former is emotional instability, the latter is stability or persistence.

In 1918 *Garnett*[9] recognised Webb's factor "w" and also isolated from Webb's material a further factor which he called "c" or "cleverness", and which has since been shown to have much in common with Cattell's "surgency" factor.

MULTIPLE FACTOR METHODS.

Garnett was the first to have used multiple factor analysis upon personality data. Much progress, however, was not achieved until these methods had been improved and were more widely applied.

[8] C. Burt: "The Factorial Analysis of Emotional Traits", *Char. and Pers.*, 1939, 7, pp. 238-254.

[9] J. C. M. Garnett: "General Ability, Cleverness and Purpose", *Brit. J. Psych.*, 1918, 9, p. 345.

McDonough (1929)[10] used Kelley's technique of analysis upon ratings of young children, to extract four factors which were named Will, Cheerfulness, Sociability and Sthenic Emotionality.

R. R. Willoughby (1932)[11] used Thurstone's earliest technique in the study of the items of the Thurstone Personality Schedule. He cast the items into six categories which he showed to have greater independence than a chance classification would have given. Analysing their inter-correlations, however, he found that there emerged only one significant general factor.

In 1933 Cattell[12] studied ratings derived from descriptions of various personality types. From these he isolated his factor "Surgency-Desurgency". For this factor Cattell retains Garnett's designation "c". He says it is so similar to introversion-extraversion as to be almost interchangeable with it. Cattell also claims to have revealed the existence of a fluency or "f" factor; and to have found that "f" correlated highly with "c" and therefore that Surgency may be measured by the objective fluency tests.

PERRY'S ANALYSIS.

R. C. Perry[13] of Southern California was during 1933-34 working upon the first extensive factorial study of a number of accredited personality tests. One hundred and seventy-eight boys and 144 girls in a junior college were given a variety of tests yielding measures of twelve qualities. Three of the measures were of intellectual ability, while nine were of personality. The personality tests used were the Bernreuter Personality Inventory scored on the original four scales, Colgate inventories of introversion and of neurotic make-up, the Pressey x-o tests of affective spread and affective idiosyncrasy and the Allport A-S reaction study. The resulting

[10] Reported by P. E. Vernon, op. cit., p. 62.

[11] R. R. Willoughby: "Some Properties of the Thurstone Personality Schedule", *J. Soc. Psych.*, 1932, 3, pp. 401-424.

[12] R. B. Cattell: "Temperament Tests", *Brit. J. Psych.*, 1933, 23, pp. 308-329.

[13] R. C. Perry: "A Group Factor Analysis of the Adjustment Questionnaire", 1934. Reviewed by G. W. Allport in *Char. and Pers.*, Dec., 1934.

table of inter-correlations Perry analysed by the three methods of partial correlation, tetrad differences and the Thurstone multiple factor technique. The results, while not identical, were comparable. Perry reports that four independent group factors are measured by the group of twelve tests. One of these is an intellectual factor, while three are non-intellectual or personality factors. Wisely Perry refused to designate by name any of the factors and Allport reports their descriptions:

(1) neurotic tendency and introversion;
(2) sufficiency, dominance, ascendance and a little intelligence;
(3) dominance and ascendance;
(4) much achievement and intelligence and a little sufficiency.

Perry found that the single test which most adequately measured the four factors was the Bernreuter—not a surprising result when one recalls the assembly of questions in that test which borrows so heavily from other tests of Perry's battery.

A set of thirty-one general attitude questions were analysed by *Whisler*[14] in 1934, to give six independent factors which seem to bear little relation to those extracted by other workers with similar material.

Somewhat similar work is reported by *Kelley and Krey*[15] (1934) who analysed tests and ratings for such traits as courtesy, honesty, loyalty, mastery, poise and fair play with a group of children. Their two main components appear to be general social conformity and individualism or assertiveness.

THE TORONTO STUDIES.

Line, Griffin and Anderson[16] of Toronto published in 1935 a report of a series of studies which had been undertaken with a view to differentiating degrees of mental stability. Their original test population consisted of fifty individuals chosen

[14] L. D. Whisler: "Multiple Factor Analysis of Generalised Attitudes", *J. Soc. Psych.*, 1934, 5, p. 283.

[15] T. L. Kelley and A. C. Krey: "Tests and Measurements in the Social Sciences", Scribner, 1934.

[16] Line, Griffin and Anderson: "The Objective Measurement of Mental Stability", *J. Mental Science*, 1935, 81, pp. 61-106.

to show in a marked and obvious way the functional gradient in which they were interested—stability. One-third of these people were graduate students of sound mental health, one-third were private patients of slightly precarious health, suffering mild neurosis, and one-third were in-patients at a psychiatric hospital. To this group ten tests were administered. These were: (1) Word association (Rosanoff), (2) Rorschach (scored only for fluency), (3) Oscillation (following Spearman), (4) Perseveration, (5) Oscillation speed, (6) Perseveration speed and (7) Bernreuter (scored on the four original scales). Two factors were isolated: factor I has been tentatively named "objectivity" while factor II is regarded as a "fluency" factor.

The year 1935 also saw the publication of two reports by *Studman*[17] of the one research upon Fluency tests. Miss Studman's analysis revealed again the factors "w" (general will factor) and "f" (fluency). The latter she compared with Cattell's Surgency factor.

FLANAGAN'S ANALYSIS OF THE BERNREUTER.

The four a priori scales of the Bernreuter (1931) Personality Inventory were submitted to analysis by the Hotelling technique in 1935 by J. C. Flanagan.[18] The four scales of the test he found to be not independent. They measured only two distinct or independent tendencies. These first two components accounted for 90% of the total variance, the other two accounting for only 4% could therefore be neglected. The first component which accounted for 78% of the variance was a compound of Neurotic, Introverted, Submissive and low Self-sufficiency scores. This seemed to Flanagan to suggest a general "Lack of Self-Confidence". He identified the second component as a "Sociability" factor. Flanagan passed out of the stage of mere analysis and made one of the earliest

[17] L. G. Studman: "Studies in Experimental Psychiatry (V); W and F Factors in Relation to Traits of Personality", *J. Mental Science*, 1935, 81, pp. 107-137.

L. G. Studman: "The Factor Theory in the Field of Personality", *Char. and Pers.*, 1935, 4, pp. 34-43.

[18] J. C. Flanagan: "Factor Analysis in the Study of Personality", Stanford U.P., 1935.

attempts to use factorial analysis constructively. On the basis of his calculated factor loadings he derived two additional scoring keys for the Bernreuter Scale, which would afford measures of his independent factors F1–C and F2–S. Bernreuter's test manual has been revised to provide scoring keys and norms for the two new Flanagan scales.

McCloy[19] (1936) applied the Thurstone analytic technique to some of Webb's results and to ratings of his own students upon forty-three traits. After the rotation of axes the analyses yielded similar results. Four factors were extracted in each case. The first of these seemed to be Webb's "w" and it consisted of all the socially and ethically desirable traits. The second factor appeared to be that of aggressiveness or domination; the third differentiated the extravert from the non-cooperative introvert; while the fourth was most significantly related with measures of health, energy and physique.

THE WORK OF J. P. GUILFORD.

During the last eight years J. P. Guilford has contributed a number of studies[20] to the factorial analysis of personality test material, particularly of the questionnaire. In 1934 the Guilfords applied to 930 students some 36 questions purporting to sample Introvert-Extravert behaviour. The application of the Spearman-Dodd test revealed that there was not one general factor. The Thurstone technique, then only in its infancy, was applied and suggested four common factors, tentatively identified as (a) social introversion, (b) emotional sensitiveness, (c) impulsiveness and (d) interest in self.

In 1936 this work was revised. Instead of the cœfficient of contingency tetrachoric correlation cœfficients were used; and

[19] C. H. McCloy: "A Factor Analysis of Personality Traits to Underlie Character Education", *J. Educ. Psych.*, 1936, 27, pp. 375-387.

[20] J. P. and R. B. Guilford: "An Analysis of the Factors in a Typical Test of Introversion-Extraversion", *J. Abn. and Soc. Psych.*, 1934, 28, pp. 377-399.

J. P. and R. B. Guilford: "Personality Factors S, E and M", *J. Psych.*, 1936, 2, pp. 109-127.

J. P. and R. B. Guilford: "Personality Factors, D, R, T, and A", *J. Abn. and Soc. Psych.*, 1939, 34, pp. 21-36.

J. P. and R. B. Guilford: "Personality Factors N and G D", *J. Abn. and Soc. Psych.*, 1939, 34, pp. 239-248.

the improved Thurstone technique applied. This new application revealed five independent factors called by Guilford: (1) S—social introversion, (2) E—emotionality, (3) M—masculinity, (4) R—rhathymia or freedom from care and (5) T—thinking introversion. The first three of these factors were well defined and tests were devised for their measurement. The latter two required further study. By January, 1939, this study was carried another step forward and in following up the factors R and T, Guilford had found D (Depression), R (Rhathymia), S (Shyness), T (Meditative thinking) and A (Alertness). Further analysis with a new group of 600 students, the reports of which were published in April, 1939, adds two more factors to Guilford's list. They are N(Nervousness or jumpiness) and GD (General Drive); and there are others awaiting identification.

LAYMAN'S ANALYSIS.

Emma Layman[21] (1937) analysed 782 personality test items from sixteen questionnaires. Reducing the number of items finally to sixty-seven and submitting them to factorial analysis she found at least twelve factors involved in this final reduced test, which was given to 276 students. After rotating the axes she identified the twelve factors as follows:

I. Sociability (1) Gregariousness.
II. „ (2) Feeling of social inadequacy.
III. „ (3) Social initiative.
IV. „ (4) Social aggressiveness.
V. Changeability of interests.
VI. Self-sufficiency.
VII. Lack of self-confidence.
VIII. Impulsiveness.
IX. Emotionality (1) Moodiness.
X. „ (2) Excitability.
XI. „ (3) Emotional introversion.
XII. Inability to face reality.

[21] E. Layman: "An Item Analysis of the Adjustment Questionnaire", *Psychol. Bull.*, 1937, 34, p. 782.

FURTHER ANALYSIS OF RATINGS—HOWIE, CHI AND REXROAD.

Howie,[22] working in the University of London, attempted an analysis of ratings of school children upon personality traits as they made themselves evident in the class-room situation. His test population consisted of nine groups of boys in the second year post primary classes of certain London and Wiltshire schools, making an effective group of 295. Both Hotelling's and Thurstone's techniques were tried, and finally the Thurstone procedure was adopted as standard. Six factors are isolated which together account for 71% of the total variance. Howie finds that the factors suggested seem to be:

I. General personality factor, a kind of all round personal adequacy involving not merely intellectual ability but social competence and emotional stability as well.
II. Forcefulness or assertiveness.
III. Likeableness.
IV. Impulsiveness.
V. Tendency to give the answer believed to be socially approved.
VI. Mainly specific to the intelligence test.

Whether or not these factors might have accorded better with other analyses with rotation of axes is not known.

Chi[23] (1937) studied teachers' ratings of pupils using the Holzinger bi-factor method. The ratings, he found, were quite significant for the study of personality. Upon analysis he discovered about one-fifth of the variance (17%) to be due to halo effect; one-third was due to a general personality factor (32%). This he identified with Webb's "w". Then he found that specific factors for each trait accounted for 47% of the variance, or about one-half. This rather large figure derives from the analytic method used, which like the Spearman technique maximises specifics.

[22] D. Howie: "Aspects of Personality in the Classroom", Unpublished Ph.D. thesis. (Manuscript lent by the author.)

[23] P. Chi: "Statistical Analysis of Personality Rating", *J. Exper. Educ.*, 1937, 5, pp. 229-245.

Rexroad[24] (1937) did a similar piece of work. He analysed ratings on ten traits for 100 girls, and found four factors necessary to account for the variance. One of these was a general factor, while three were broad group factors.

FACTORIAL STUDIES BY LURIE AND MOSIER.

An outline of Lurie's[25] study is best given by himself. "In an attempt to determine stable categories for the classification of personality with regard to value-types, a test blank was written with 144 items classed according to Spranger's system and was administered to 203 freshmen and sophomores at the University of Chicago. It was scored in such a way as to yield four measures corresponding to each of Spranger's six types for each subject. Tetrachoric correlations were obtained and a factor analysis was performed by the Thurstone centroid method. The orthogonal factorial matrix was subjected to a transformation maximising the number of small entries, thus forming a matrix of oblique co-ordinates in which seven patterns stand out very clearly."

The four basic clusters of items or four basic attitudes were found to be:

I. The Social type which values especially human relations.
II. The Philistine type which emphasises utility and power at the expense of beauty and harmony.
III. The Theoretical type which stresses truth and cognitive values.
IV. The Religious type which emphasises spiritual life and values.

Three less important factors are:

V. Open-mindedness,
VI. Practicality and
VII. An æsthetic attitude.

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[24] C. N. Rexroad: "A Factor Analysis of Student Traits", *J. Educ. Psych.*, 1937, 28, pp. 153-156.

[25] W. A. Lurie: "Study of Spranger's Value Types by Factor-Analysis", *J. Soc. Psych.*, 1937, 8, pp. 17-37.

Lurie believes that a more plausible and self-consistent system of personality classification can be founded upon his first four types than upon the six which Spranger developed by intuitive analysis of experience.

Also in 1937 Mosier[26] published a report of a research into the thirty-nine most significant items of the 223 of the Thurstone Neurotic Inventory. The results of the study indicated that these selected items contained at least eight traits which together account for 51·2% of the total variance. After rotation of axes Mosier's eight "traits" are given names such as "cycloid", "depression", "hypersensitivity", "inferiority", "social introversion", "public self consciousness", "cognitive defect", "autistic tendency".

ANALYSIS OF QUESTIONNAIRES—VERNON.

Boyd's (Glasgow) Personality Questionnaire (unpublished) has, according to Vernon,[27] been widely used in Britain. Vernon, himself, has submitted the results of 50 men and 50 women students on this questionnaire to analysis by the Thurstone technique. The Boyd test yields scores on nineteen a priori scales. Vernon studied the inter-correlations between these 19 sets of scores; and he found that a general factor accounted for the first 41% of the variance, while the next three factors brought the total to 76% of the variance. Here he stopped, and concluded that the 19 measures are far from distinct. After three rotations of the axes he attempted tentative identification of the factors as under:

I. Psychoneurotic tendency, heavily weighted with depression, instability, anxiety, lack of self-control, and lack of self-sufficiency.

II. Care-freeness, weighted with suggestibility, shirking responsibility, freedom from worry and from self-consciousness, and lack of definite interests.

III. Scrupulousness. Measures with highest loadings were—obsessional carefulness, freedom from instability, ready action, freedom from emotional thinking, strong self-control and suspiciousness.

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[26] C. I. Mosier: "A Factor Analysis of Certain Neurotic Symptoms", *Psychometrika*, 1937, 2, pp. 263-286.

[27] P. E. Vernon, op. cit., pp. 71 and 74.

IV. A sex difference factor which suggested as feminine traits—dislikes, fears, instability and dependency; and as masculine traits—suspiciousness, inability to concentrate and introspectiveness.

WEBB'S DATA REVISED.

From Capetown comes a further analysis of some of Webb's data by Reyburn and Taylor.[28] Data pertinent to nineteen of Webb's traits are selected for further study by the Thurstone technique. A correlation matrix extracted from Webb's report is submitted to a factorial analysis in which four factors are found to be sufficient to account for most of the variance. Reyburn and Taylor rotated their axes, not to obtain simple structure, but in order to make at least some of the axes correspond to well-defined components of the data. Garnett and Cattell had both found a factor "c" in this material heavily weighted with items 1 and 8. These workers, therefore, pass one axis through trait 1, and so on. As a result they find a first factor akin to Cattell's Surgency-Desurgency though not necessarily identical with it. Their second factor they designate Perseverance, pointing to its relation to Webb's "w" from which it differs chiefly by giving more weight to steadiness, continuity, and perseverance than to action from principle or purpose.

The third and fourth factors, however, are relatively new. The third they call charity for its heavy loading with such traits as tact, recovery from anger, kindness, conscientiousness etc. The fourth quality, which is unipolar, has been called Social Sensitiveness, which is not to be confused with sociability or gregariousness. It is determined by its weights with such traits as corporate spirit, tendency to kindness, trustworthiness and conscientiousness.

TWO SYDNEY STUDIES.

During 1939 the present writer conducted two factorial studies in an attempt to obtain independent and coherent personality "traits". The first of these studies applied the Thurstone technique to the results of 200 University students upon twenty-two variables. These included the Allport A-S

[28] H. A. Reyburn and J. G. Taylor: "Some Aspects of Personality", *Brit. J. Psych.*, 1939, 30, pp. 151-165.

test, the Personal Inventory,[29] the Bernreuter personality inventory, Otis Higher Examination, Five fluency tests, Two perseveration tests, tests of attention, concentration, free association; also age and sex. Four factors are found to account for 51·2% of the total variance. In interpretation these four factors seem to be (1) a general factor among the inventories—probably best called by Flanagan's term, "Self-confidence"; (2) a second factor from the inventories—again probably Flanagan's factor of "solitariness"; (3) a fluency factor; and (4) a factor which from its relation to the tests may be designated "concentration".

The second study pursued the analysis of the personality inventories still further. One hundred scores on the Personal Inventory were used in a factorial study of the item intercorrelations. This analysis was carried to three factors which it was found could be identified with similar factors isolated by Guilford using quite different material. The factors appeared to be: (1) Emotional depression, (2) Emotional immaturity or dependency, and (3) Sociability or shyness.

CONCLUSION.

The conclusion to be drawn from such a detailed summary of research projects is plain. Identical factors are not yielded by all studies. Thus those factors which are isolated remain hypothetical. And yet in the words of Vernon (p. 76): "The majority agree in showing the predominating importance of one factor, which may be interpreted as lack of self-confidence, or instability of personality. Several also show an independent sociability factor. When both men and women are tested there is a sex difference factor. And Guilford and the writer (Vernon) seem to concur on a 'carefreeness' factor."

Considering the variation among the "tests" and test programmes used, and among the subjects of the studies, the likenesses of results are more striking than the differences. There is a definite promise of a limited number of independent, coherent, homogeneous "traits" at least among self-analyses of personality, by questionnaire or rating scale.

(To be continued.)

[29] 42 questions prepared by A. H. Martin from the Thurstone Personality Schedule.

MOTIVES AND BEHAVIOUR.

By K. F. Walker.

In a recent article in *The American Journal of Sociology* (3) Professor R. M. MacIver has argued strongly that a complete explanation of social behaviour must include the imputation of the "motives" of the agent, and that such imputation is not impossible or unscientific. Such a plea is against the prevailing trend of thought in social psychology, but the question has never been stated in a form which is free from verbal confusion, and it seems worthwhile to re-examine this thorny point of controversy in the light of our increasing sophistication in the use of language.

The case for the inclusion of "motives" in the causal equation for behaviour has been most vigorously stated by McDougall (4), who made it the cornerstone of his whole theory of behaviour. Rather than re-examine the numerous controversies in which McDougall participated in his life-time, we shall take as a basis for discussion Professor MacIver's statement of the view in question, since this is less widely known among psychologists and therefore less likely to arouse old-established negative attitudes and prejudices.

Professor MacIver discusses two objections which are commonly made to the contention that a complete account of behaviour must include the imputation of the agent's "motives". The first is that a "motive" is not an overt action, but "a purely subjective factor that cannot be exposed to any kind of direct scrutiny" (3, p. 2), and the language of "motives" is therefore "animistic" and "folklorish". The second contention is that "motives", being subjective factors, cannot be scientifically observed. Two difficulties are said to be in the way. The first is that his "motive" is "the secret of the agent" (3, p. 4), and "even if he is genuinely attempting to reveal it he cannot offer us any proof. There is no way of 'objectifying' the motive. It cannot be recorded; it cannot be expressed in precise, unmistakable symbols. It cannot be made amenable to tests of its presence or absence, still less of its

quantity and degree" (3, p. 5). The second difficulty that is alleged to be insuperable is that the agent himself, as the Freudians have demonstrated, can have "no assurance concerning his motives" (3, p. 5).

Clearly, if these contentions are true, the imputation of "motives" can be dismissed as outside the scope of psychological science. It then rests with those who deny that the scientific imputation of "motives" is possible to demonstrate that behaviour is completely explicable without the imputation of "motives".

To the objection that it is unscientific to speak of "motives" at all, since "such concepts belong to a scientifically outmoded, animistic, folklorish manner of speech" (3, p. 3), Professor MacIver replies that such an attitude is blind to a part of reality. "We cannot dismiss, as beyond scientific enquiry, any intelligible question concerning reality. That motives belong to the world of reality is established by the best of all evidences—that of immediate experience. We are all aware that we have motives" (3, pp. 2-3). To neglect the study of "motives", therefore, "would exclude from reality something indubitably present in human behaviour". He charges those who hold that "The apparent role of motives as determinants of behaviour is a subjective illusion" (*loc. cit.*) with failure to establish their contention. Finally, "even if it were established, these illusory motives would still remain among the conditions under which behaviour takes place" (*loc. cit.*).

This reply is beside the point. Those who find the concept of "motive" useless in the explanation of behaviour do not deny that "motives" exist. "Motives" should certainly be investigated, and the laws of their occurrence determined, just as much as any other behaviour. The issue is whether "motives" play the role which Professor MacIver attributes to them in the determination of behaviour.

The last citation betrays a confusion of the phenomena to be explained and the events which are the *conditions* of their occurrence. In another place Professor MacIver writes, "among the *conditions* of human behaviour are the subjective

attitudes and impulses without which it would certainly not be human" (3, p. 3). This is simply a flat assertion that "motives" help to determine behaviour. Professor MacIver seems to think that if behaviour is always accompanied by the occurrence of "motives", the "motives" must be causally related to the behaviour (i.e., be a *condition* of its occurrence). Suppose the behaviour to be explained is the killing of one man by another. Now Professor MacIver says that among the *conditions* necessary to the occurrence of the killing are the killer's subjective attitudes and impulses, his "motive". This implies that if the killer's "motive" had been different, or if he had had no "motive", the killing would have occurred differently, or perhaps not at all. It is incumbent on Professor MacIver to tell us what difference the absence or difference of "motive" would make. When he says that it would remove the human quality of the behaviour, it is hard to avoid the suspicion that he means merely that it would remove the "immediate experience" concurrent with the action. In short he is at once explaining the killing by the killer's subjective attitudes and impulses, and including those impulses in the killing behaviour as the human quality in it. The "motive" cannot be regarded both as a quality of the phenomena to be explained and as a condition of their occurrence.

So far, then, we are given no evidence that the imputation of "motives" is necessary to the explanation of behaviour, although we are assured that "motives", being an aspect of behaviour, must be included within the scope of psychological observations. The second objection which Professor MacIver discusses is really a denial that it is possible to observe "motives" scientifically; it is therefore concluded that they are beyond the scope of psychology, either as data to be explained, or as concepts in terms of which behaviour is to be explained. Those who raise this objection usually undertake to provide a complete explanation of behaviour without reference to "motives".

The first contention in support of this objection is that "motives" are by their very nature private to the agent. Professor MacIver denies this, and argues that it is possible

to impute "motives" from their manifestations. In this respect, he holds, our knowledge of "motives" is of the same character as our knowledge of causation, which is always inferential. In individual instances, he says, we may mistake the "motive" of the agent, but "since we have hundreds of instances we can discover with a high degree of assurance typical social behaviour under well-defined conditions. And in this typical behaviour we find quite recognisable motives operative" (3, p. 7). The difficulty that the agent may misrepresent his "motives" intentionally is overcome by considering the consistency of his report with his life-history and other actions. "But when we relate the single action of the individual to his other actions, to his life-history, and still more when we relate the action of one individual to the actions of others in similar situations, we may well discover a consistency of behaviour that discounts the diverse allegations of plausible motives and enables us to discern with high probability, if we proceed far enough, the characteristic motivation associated with types of situation" (*loc. cit.*). Professor MacIver disposes of the further objection that the agent himself may be mistaken about his own "motives" by pointing out that the objection is valid only in so far as "motives" are knowable. Otherwise we could never assert that the agent was in error. We discover him to be in error by reading "the indices of motivation" (3, p. 11) within the coherence of total situations, though Professor MacIver appears to be uneasy about the application of the term "unconscious motives" to the factors which the Freudians and others have shown to be at the back of our actions. "In what sense, if any", he writes, "can we be said to have desires, feelings, attitudes, of the existence of which we are not even dimly aware?" (3, p. 10).

Our account of MacIver's position has brought us face to face with the fundamental issue on which the solution of the whole problem turns, viz. the definition of the term "motive", and we shall commence our examination of MacIver's thesis with a consideration of this issue. The sense in which Professor MacIver uses the term "motive" has

already been indicated. By the agent's "motive" he means "a purely subjective factor" (3, p. 2), "the subjective attitudes and impulses" concurrent with, or preceding, the action. His use of the term thus corresponds closely with the common-sense usage of "purpose", and it is probably a fair statement of Professor MacIver's concept to say that the "motive" of a man's action is his awareness of the goal towards the achievement of which the action tends. It is important to note that this goes further than saying that human behaviour is typically *directed* towards certain goals, or end situations, for this latter statement says nothing of the agent's awareness of his goal, and there is nothing in it to distinguish man from animals or even inanimate objects. Iron filings move towards a magnet: water runs down hill. There is no difference between these statements and the statement that the human (or animal) organism when deprived of food changes its relation with environment until it gets food. In each case the behaviour is *directed* towards an equilibrium situation. For Professor MacIver, the "motive" of the behaviour is the agent's awareness of the end situation.

In examining the adequacy of this concept, let us begin by considering how we come to know that an organism is aware of something. We come to know it by observing some kind of consistent discriminatory reaction on the part of the organism to a differentiated environment. We may ask a man to tell us whether a certain light is green or red, or we may get him to push one key if it is red, and another if it is green. In either case he is making a different response in a situation which differs only in one particular, the redness or greenness of the light, and we infer from this difference in response to a different environment that he was aware of the difference between red and green. When a verbal report is not feasible we infer other people's awareness of things from their discriminatory responses. "Animals, children and irresponsible adults are recognised as conscious only in as far as they discriminate, that is to say, as they react differentially (discriminatorily) to a differentiated situation. The contents of

the unconscious mind can be asserted to exist only in as far as they appear in differential response" (1, p. 450).[1]

We have said that behaviour is essentially goal-directed. It should be emphasised that this does not mean anything more than that the organism reacts differentially to the different situations in which it finds itself, striving to change some and maintain others. Behaviour ceases when certain typical relationships with the environment are maintained and these organism-environment complexes may well be called the "end situations" or "goals" towards the establishment of which the organism's behaviour is directed.[2] When we say that behaviour is goal-directed, then, we are asserting that it makes a discriminatory response to a differentiated situation, the differentiation consisting in the different implications of the various organism-environment complexes for the gratification of the organism's needs. This is our only criterion of awareness, and there are no grounds for not saying of goal-directed behaviour that it involves awareness of the goal. But this is not the same kind of awareness as Professor MacIver has in mind, for he says that it is this awareness which separates man from the animal kingdom, let alone from inanimate matter. But the discriminatory response, as we have seen, is not confined to men or animals. Water gives a discriminatory response when it runs down hill; iron filings discriminate magnet from non-magnet. If consciousness be defined as "awareness of an object", we cannot avoid attributing consciousness to iron filings, for they display the only criterion of such awareness that we have (1, p. 456). In face of this, no doubt most of us will prefer to give up using the term "consciousness" in this sense.

It appears, then, that Professor MacIver's "subjective attitudes and impulses" cannot consist simply of awareness of the goal of the action, in any intelligible sense of the term "awareness". They must involve awareness of the awareness of the goal. "The awareness of the awareness of an object" is

[1] My debt to Boring's paper is very great, and the reader is referred to it for a fuller exposition of the position adopted here.

[2] See Tolman (5) for fuller statement of this view.

the other traditional definition of consciousness, which does practically limit consciousness to man. Now here, as Boring points out (1, pp. 457-8), the crucial point is how we are aware of our awareness of an object. He cites as an example the activity of running the eye down columns of surnames arranged in random order, to note all the *Smiths*. The inspection may be very speedy but without error. Every *Smith* is perceived. But is every other name perceived as *not-Smith*? Yes, in the sense that unless the reader had been aware that the names were *not-Smith,* he must have made an error (i.e. he unconsciously discriminated between the *Smiths* and the *not-Smiths*). "What is the difference between the *Peterson* that goes unnoticed and the *Smith* that is noticed? It is possible for the organism to react correctly to his reaction to *Smith*; he knows that he reacted; he is aware of having been aware. No such statements can be made about *Peterson*. The best that the organism can do about *Peterson* is to read the list slowly again, single out the name, and infer that he must have reacted to the word because he was correct in not noting it" (1, p. 458). This inference could have been made by the external observer just as accurately from the data in his possession. He could also have inferred that the reader had been aware of all the *Smiths*. He has, therefore, just the same degree of knowledge about the reader's awareness of objects as the reader himself.

This is true of all cases where we are not sure of our own "motives". What we do is to use the same methods of inference as are used by the external observer and our conclusion has no more validity than his. What of the cases when we do feel sure of our motives? It is true that in Boring's example the reader is aware of having been aware of the *Smiths,* but his awareness is again not immediately given, but inferred, as in all experience. It was pointed out long ago by James (2) that we never have experience of the present, but only of the just past.[1] Our awareness of being

[1] "The attempt at introspective analysis in these cases is in fact like seizing a spinning top to catch its motion, or trying to turn up the gas quickly enough to see how the darkness looks" (2, p. 244).

aware of our goal is therefore always an inference of having been aware. But the crucial point is whether it is a private or a public inference. As Professor MacIver has pointed out, and the psycho-analysts have demonstrated, our inferences are often wrong, that is, we infer that we were aware of a goal which is not the goal of which we were aware at all. To avoid verbal misunderstanding here, let us call the awareness of the goal, defined by discriminatory response, the organism's *objective*. Professor MacIver's "motive" is then "awareness of the objective". This awareness we have seen to be an inference, which may be wrong. The Puritan believes his objective to be righteousness in others and himself; the psycho-analyst says that his objective is to keep his own impulses under control by punishing them vicariously in other people. His inference of his objectives is wrong, though he is not aware of its incorrectness. The hypocrite, on the other hand, is aware that the inference he makes in public is invalid. We know a man to be a hypocrite only in as far as we too discover this invalidity and his awareness of it.

There is no guarantee, then, that one's own inference of one's objectives is any more accurate than anyone else's inference of them. In many cases there is reason to suppose that one's own inference is likely to be less accurate. As Professor MacIver says, we have to "read the indices of motivation within the coherence of . . . total situations" (3, p. 11).

So far nothing has been said about the imputation of the agent's *motives*. Professor MacIver replies to the contention that motives cannot be scientifically studied by showing that we habitually do make inferences from people's actions to the forces moving them, and argues that our recognition of the possibility that the agent may make a false inference implies that a correct inference is possible. This argument is perfectly valid, but the methods which Professor MacIver prescribes for the imputation of the forces determining behaviour will not lead us to "motives" as he defines them. He comes close to recognising this when he queries the application of the

adjective "unconscious" to "motives". Such a combination is clearly inconsistent with his whole concept of "motive". The psycho-analyst does not infer the operation of unconscious *motives* but of unconscious "needs", or "*drives*". The term "need" may be used to signify the fact that the organism's behaviour is goal-directed. To say that the organism has a hunger need is simply to say that when deprived of food for a certain period of time it changes its relationship with the environment until food is ingested. It makes no reference to the organism's knowledge of the objective of the need, the ingestion of food, this question being left open. In the concept of "need", therefore, we have an explanation of the "consistency of behaviour that discounts the diverse allegations of plausible motives" (3, p. 7). Professor MacIver has simply described the criteria commonly used to infer the operation of particular needs. These criteria will enable us to make statements of the type, "Under certain conditions, certain behaviour occurs". Such a statement says nothing of the agent's awareness of his objectives. These criteria therefore will not enable us to infer the agent's motives, constituted of his *awareness* of his needs. Professor MacIver gives us no clue as to what might be satisfactory criteria of the presence of motives as he defines them. If a man tells us that his motive was to preserve the happiness of others, and we cannot catch him in any intentional misrepresentation, his statement is true, whether or not his objective (of which he was unaware) was in fact to save his skin.

This problem has never been squarely faced by those who affirm that the imputation of motives is necessary to the explanation of behaviour, perhaps because they have been so entangled in verbal ambiguity as never to get it clearly stated in this way. The confusion of "purposive" in the sense of "goal-directed" and "purposive" in the sense "with foresight of goal" has been extraordinarily persistent. McDougall (4) appears to have seen the distinction perfectly clearly, but failed to understand why Tolman, Perry and others should prefer to use "purposive" exclusively with the former meaning.

and to describe behaviour solely in these terms. It is true that at the time he wrote, the reasons against taking the two senses of "purposive" to be equivalent, as McDougall urged, had not been as clearly stated as they might be stated now. The point is that our behaviour may be goal-directed without our knowing its goal, and that our knowledge of its goal, not being immediately experienced but inferred, is of the same logical status as the external observer's knowledge of it. When we add to this the psycho-analyst's evidence that we often have good reasons for making wrong inferences, a preference for separating the two meanings of "purposive" requires no further justification.

McDougall comes close to recognising these points when he writes, "the justification for doing so [i.e., using "purposive" to mean "with foresight of goal"] is (i) that when I myself so act, I know, *if I stop to reflect,* that I foresee the goal and desire it; and (ii) that if I question other intelligent persons, they tell me that, when they act in this objectively purposive fashion, they also foresee the goal and desire it" (4, p. 300; my italics). The fact that we have to stop to reflect is the significant point.

There still remain the questions, is the study of "motives" possible by scientific methods, and if so, is it necessary to the explanation of behaviour? The crucial point is whether the existence of a motive makes any difference to the behaviour. For unless it does we can only have recourse to the report of the agent, which may not be forthcoming, and is not available in animal psychology.

Clearly the "motive" (awareness of the goal) can have no effects on the behaviour when the action is past, and the agent infers what his "motive" must have been from thinking over the past happening. This retrospective imputation of "motives", whether done by the agent or the external observer, is only retrospective imputation of *objectives.* The agent's present knowledge of his past *objective* can have had no effect on the behaviour when it actually occurred.

When the objective has not yet been attained, however, what difference does the agent's awareness of it make? McDougall begs the question when he says that the ascription of foresight of goals to Köhler's apes is an apt mode of description. If we ask why, we are told simply that we have foresight of goals in similar circumstances, which brings us back to the point already considered, as well as being an argument by analogy.

The demonstration of both the possibility and the necessity for the imputation of motives therefore rests upon the demonstration that awareness of the goal has an observable effect upon behaviour. Some such criterion of the presence of motive does seem to exist in the much greater degree of directed variation of response when habitual responses are ineffective. A hungry infant merely thrashes about if refused the breast; an older child, if the usual method of getting food fails, will immediately try another, which is not selected at random but is closely directed towards attaining the objective. One reason why the infant cannot make such a specifically directed response is because he is unaware of the essential character of the relationship with the environment which relieves his hunger need. The criterion of purpose, then, is the degree of plasticity of the behaviour, from which we may infer a certain "insight" into the situation. When the learning curve shows a sudden drop rather than a steady decline in time and error, we may postulate that the organism is aware of its objective. Awareness of the objective increases the organism's power of adjustment.

We must not describe the behaviour of Köhler's apes as being "insightful" because in similar circumstances *we* have awareness of the objective. This is unwarranted anthropomorphism, and neglects the fact that our awareness of the objective must be checked before we can be sure of its accuracy. The point is that we check on the accuracy of *anyone's* statement that he is aware of his objective by the criterion of whether his actions do further his objective, and whether if his habitual mode of attaining his objectives is not effective, he

manifests "insightful" alternative responses, rather than purely "random" responses. If a man says that he bashed his wife's brains out in order "to stop her from worrying", we can see that the effectiveness of his actions in that direction was not increased by awareness of his alleged objective, and we can say with great assurance that his motive was false. We must apply the same test to ourselves, to guard against the ever-present danger of rationalisation. There is, then, no question of our reading our feelings into the behaviour of others, or of animals; we merely use the same method to infer the presence of a "motive" in the other person as we use to infer its presence in ourselves.

It is true that this criterion of the presence of motive is as yet vague, and may be present in varying degrees. Further experiment may succeed in defining it more sharply, and it is suggested here only as an indication of the direction in which the answer may be found to the question, "Is the imputation of motives necessary?" The important thing is that we are often unaware of the forces determining our actions, and that our knowledge of these forces, being an inference, is of the same logical status as the external observer's knowledge of them.

If we hold firmly to this point of view, we shall be forced to alter our ways of conceiving the whole problem of motivation, using that phrase, in its fullest sense, to mean the problem of explaining behaviour. "Motivation" has, indeed, become an obsolete term if "motive" is defined as it has been in this paper. "The theory of motivation" is no longer a synonym for the "explanation of behaviour", unless we extend the meaning of "motive" to make it signify "anything that moves the organism". Even then the term has an unfortunate penumbra of suggestion. The notion that behaviour is to be explained in terms of agents within the organism, which pull and push the organism about, is responsible for the long-persisting confusion of the two meanings of "purposive", and the consequent failure to state the psychological problem clearly. Even the term "need" is too one-sided, locating the

origin of behaviour in the organism instead of in the organism-environment relationship. It is the great merit of Lewin's concepts that they go some distance towards conceiving of behaviour in terms of changing relationships *with* the environment rather than in terms of the organism behaving *in* an environment which merely provides a background for its actions.

It seems desirable, therefore, that the term "motive" be dropped from psychology (and other social sciences). Let us speak without ambiguity of the needs of the organism and of the organism's awareness of them. If it is desired to focus attention upon the end-situation towards the establishment of which behaviour leads, the term "objective" (or some more neutral term) might be used. The organism might then be said to be aware (or unaware) of the objective of its need. The criterion of such awareness is the degree of "insight" apparent in the organism's behaviour, manifested by sudden drops in the learning curve and by directed plasticity of behaviour in face of frustration. Further experimental research may enable us to define this criterion more sharply.

SUMMARY.

Those who deny the usefulness of the concept of "motive" for psychology commonly bring two arguments in support of their view. The first is that the whole notion of "motive" is "animistic" and "folklorish", since a motive cannot be directly observed. The second is that "motives" cannot be accurately observed, and therefore are beyond the scope of scientific study, because (a) they are "the secret of the agent", and (b) the agent himself has no indubitable knowledge of his "motives".

In a recent article, Professor MacIver defends the view that the imputation of "motives" is necessary to a complete explanation of behaviour. To the first of the above criticisms, he replies that such a view neglects a part of reality and that since "motives" exist they should be studied by psychology. This reply does nothing to demonstrate the necessity of imputing motives in order to obtain a complete explanation

of behaviour. To the second criticism he replies (a) "motives" are not the secret of the agent, and (b) we can only assert that the agent has mistaken his "motive" if we can gain independent knowledge of it. We can do this, Professor MacIver asserts, by observing typical behaviour in typical circumstances, and making inferences from the coherence of total situations.

Professor MacIver's replies to the second of the above criticisms are valid only if "motive" is defined as equivalent to "need", so that we can say "under such-and-such conditions, such-and-such behaviour occurs". A statement of this type says nothing about the agent's awareness of his goal. When the term "consciousness" is carefully defined along the lines suggested by Boring, the methods of imputation described by Professor MacIver can be seen to lead us only to the agent's "drives", or "needs", defined without reference to the agent's awareness of his goal. Professor MacIver uses "motives" to mean "awareness of the goal", but gives us no clue as to how they may be imputed, and adduces no proof that their imputation is necessary.

A possible criterion of the presence of a "motive" is the degree of direction observable in the organism's varied reactions in response to frustration, though the more precise definition of such a criterion must await further experiment.

In view of the ambiguity of the term "motive" and its penumbra of false suggestion, it is desirable that the term be dropped from psychology.

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NATURE AND PRECISION.[1]

By John C. Begg.

Precision has usually commended itself as a virtue. At infant school we were told to toe the line; but this first inculcation of precise order might, had we observed more closely, have revealed the limitation that toes in line probably meant heels somewhat out of alignment. So in modern physics one encounters the Heisenberg principle that an attempt to locate a moving particle with complete accuracy inevitably renders indefinite the assignment of its momentum. The controversy over causality which has flared up in recent science is really only one phase of the more general question of precision or definiteness in Nature—an unchallenged assumption formerly underlying theory as an unconscious postulate. Of course an important difference always holds between loose and rigorous thinking or description; but do we in seeking absolute accuracy pursue a chimera? Further, is there a politic limit to such a search—a region of maximum lucidity beyond, as well as before, which the mists of confusion gather or impotence supervenes? This topic has been much to the fore in recent philosophical discussions—as witness, for example, articles in the American journal *Philosophy of Science* headed "Vagueness and Logic", "Science and Vagueness" etc. The import of the subject in science was emphasised in my mind as I listened to the presidential address in the Physics section of the British Association for the Advancement of Science, as delivered by Dr. Darwin at Cambridge in 1938, in which he contended that future advances in physics lie along the path of approximation and probability concepts and methods.

[1] From an address delivered to the Philosophical Club, Dunedin, in 1939.

Heisenberg's Principle of Uncertainty, just referred to, may seem to be a small part in physics—an exception remarkable for its singularity—but an exception is apt to be important, not chiefly by its subject-matter, but because it may be symptomatic of radical misconception in a whole theory. Such was the case with the erratic element in the orbit of the planet Mercury shown by Einstein to import a complete change in the scientific conception of gravity. We may reasonably inquire, then, if ultra-precision is the real key to the scheme of Nature. Jules Verne's hero of accuracy, Phileas Fogg, dismissed his valet for bringing shaving water of a temperature differing by four degrees from that stipulated, but presumably even this paragon would tolerate a variation of one degree or a fraction of one! Shakespeare's Macbeth reflected on the anomaly that

> Hounds, greyhounds, mongrels, spaniels, curs,
> Sloughs, water-rugs, and demi-wolves are cleped
> All by the name of dogs.

General terms always imply some margin of looseness in application, and so Plato could find satisfaction only in the archetypal ideas laid up in heaven. The actual empirical investigations of science seem inconsistent with a rigidity excluding all ambiguities. Nature must be approached with pincers not already closed, but with jaws somewhat open for the next bite. Mr. A. C. Benjamin of the University of Chicago has said: "Vague ideas are part and parcel of science, and any theory of meaning which denies them is not a theory of science" (*Philosophy of Science,* October, 1939).

Mankind, in the advance from a primitive condition, has varied in his confidence regarding the possibility of framing Nature in precise lines. First the idea of order was confined to a narrow application in the immediate and familiar environment: within the walls of the city a measure of control could be based upon a fairly certain and dependable behaviour of the materials upon which daily life and industry rested (although even there occult intrusions seemed to damp any great confidence in rigid law); but out in the wilderness,

mountains and distant places, confusion and chaos reigned supreme. To extrapolate by a rigorous line of thought from the known to the unknown was altogether too precarious. Later a little more boldness was exhibited by thinkers; e.g. a small triangle was drawn, and its properties, when established by close study, were confidently attributed to all similar triangles in the Universe, however large or small. Archimedes undertook to move the world if provided with a fulcrum and a sufficiently long lever. But even that philosopher did not dare to say that he, and every other man, actually did move the world (albeit by ever so small a fraction) every time he stepped along the road. Outside the region of immediate sensible discrimination it seemed precarious, or at least inappropriate to proceed. This attitude remained a background down to modern times. The idea that properties based upon refinements of observations within small compass could be extrapolated to amazingly intricate machines, or to vast and remote regions, as in astronomy, was long in being grasped and exploited. Then, when at last this notion was appropriated and used with marvellous success, confidence in it was unexpectedly shattered by a new discovery. In regions extremely remote from sensory observation, on the sides of both minuteness and immensity, the well established, clear-cut laws and ideas completely broke down. A single entity appeared capable of putting on diverse natures at random, and the fabric of time and space themselves seemed to be subverted and ruptured. I cannot attempt to elaborate the manner of this: the fact is now well-known. One might ask in trepidation and alarm if we have returned to the primitive outlook. The walls of our law-controlled city have been far expanded; but do we still look outward to the chaotic jungle or the clouded mountain tops where inscrutable deities wield their capricious thunderbolts?

Another suggestive fact is that our ultimate standards of exactness are now seen to assume a rather ambiguous character. The solid reference bars, preserved with meticulous care to sustain the uniformity of the yard and metre, are not

merely dancing atoms, but more correctly oceans of tumbling waves. Again, the stars by which we check our time are really hurtling through space at imperfectly known velocities as they are played upon by a multitude of unassessable forces. Their apparent stability or regularity is an optical illusion resulting from their immense distance from us—just as a far away ship at sea looks stationary.

The everyday conception of the world regards it as a set field of objects which may be investigated with any degree of precision we may choose. Although we may fail in arriving at the absolute and unsurpassable truth about the objects, that is a weakness of thought or method, leaving the fixed structure and elements of the world still standing in unimpeachable integrity, and offering the prize of final success to the intellect active and acute enough to penetrate the secret recesses. Closer consideration and experience may show that, whatever its metaphysical or ultimate status, the manifold of objects as we know it is more like the composite shadow of our own minds, containing features so elusive that in the very act of our pointing their place they flit away. We are left to ponder whether in this flux the notion of precision can find any exemplification whatever. Certainly there is a difficulty in carrying forward a past (and therefore foreign) standard into a context of ever-welling novelty; and yet precision must mean the coming up to some established standard in its full import. We encounter here the age-long antithesis between the one and the many. Early in our line of culture the Greeks discerned something unsatisfactory in the vague and moving manifold of Nature—something that could not be made to correspond with the static serenity of the ideas and terms by which the manifold was scanned and characterised. Widely diverse things were subsumed under a common term. The valorous soldier with his blood-stained sword, and the philanthropist feeding a hungry child, were alike called virtuous. Where in such confusion could rigorous thought or philosophy find a resting place?

A refuge from such considerations as the foregoing was found by the Greeks in geometry, devised earlier by the Egyptians for the apportioning of land, but now seized on as a steadfast frame in which, peradventure, the ungoverned turmoil of the changing scene might yield up to view some modicum of rationality and reality. The distillation of the static and enduring, as opposed to the churnings of the phenomenal world, formed the basis of the classical tradition which the medieval scholastic philosophers inherited and transmitted to us. Clear-cut incorruptible ideas, and formal Aristotelian logic admitting no loophole of doubt or uncertainty, were diligently sought; and anything falling short in these respects was condemned.

The extent and depth of the pervasion of Greek thought in the whole modern outlook, including science, is difficult to assess; but it must be remembered that the Greeks themselves were by no means unanimous regarding the attitude which has become dominant in tradition. Some of the schools dissented, and were indeed contemptuous of the philosophers of the One, the stable and the precise, maintaining on the contrary the supremacy of the many, the flux, and the contingent; and this antithesis has asserted itself also in European culture, especially in later times. Doubtless in practice hosts of people have always ignored the preaching of philosophers who exalted the rigours of reason and abstract entities against the swaying vicissitudes in which common humanity is apt to find the spice of life; but for long few were found to embody this attitude in philosophical terms.

A new phase arose with the well-known story of the rise of inductive methods and the gradual discrediting of the ideal and 'a priori'—at least as far as these were implied in the investigation of Nature. Nevertheless, a long period has ensued during which a modus of give and take has been current, as between the empiricist and the formalist. The empiricist could not disregard deductive methods, nor could the classical logicians challenge the new induction as a mode of inquiry and an eliciter of truth (in some sense). Of course induction

is a shocking case of imprecision. No method of induction is infallible save that of 'simple enumeration', which can scarcely be called induction proper: and up to the present time nobody can give a satisfactory reason why induction should be valid at all. While it is probably true that intellect could scarcely function normally in a world where inductive inference proved usually invalid, intellect can, and in fact does, function normally in our world where induction is at best precarious. Here was a tremendous hurdle for the apostles of mathematical and logical rigour. They thought they had possession of the keys of the unseen world, on which the stream of phenomenal Nature must ultimately depend. Further, they thought that a sufficiently searching logical analysis could reveal more and more of Nature itself. Hegel inherited the essence of this attitude, and, despite the rising tide of inductive science in his time, he virtually construed the material universe as congealed logic, and ventured to predict that no new planet would be discovered because the then known number was a perfect one!

I am taking up some time with these fragmentary historical remarks, not merely for their philosophical interest, but because they lead on to the matter of the recent change in the outlook of science, which is merely a further step towards breaking down the sacrosanctity of preconceived theory as applicable to the empirical phenomena of Nature. Dr. Darwin, in the lecture already referred to, said: "The history of the development of physics in the first quarter of the twentieth century will rank as one of the greatest in the advancement of knowledge, but it will also rank as one of the most curious in the history of thought."

We are apt to think of the break with Aristotelian scholasticism as brought about rather suddenly by such men as Francis Bacon and Galileo, but in fact there were many actions and reactions. Scientific thought and method, not being themselves very securely based on reason, have had need of the more stable and precise background of classical 'apriorism', which need, although the satisfying of it is not

unattended with dangers, is really imperative. Some metaphysical presuppositions and 'a priori' logical method are inseparable from any intellectual activity, scientific or other. To refer to Dr. Darwin again, he quotes Professor Ehrenfest of Copenhagen as saying: "To believe that one can make physical theories without unobservable quantities—that is one of the diseases of childhood!" The truth is that metaphysical 'a priori' ideas and methods are apt to be, and have in fact been, very much intertwined with empirical procedure in the investigation of physical Nature. Two notable instances of this are presented by geometry and causal action. We may trace the idea of precision in the case of geometry as applied to science. Geometry was a trump card in classical times, and its rigour and precision have given it a high place in intellectual activities down to the present day, although the dissecting knife of criticism has lately shorn it of its former halo. Here was something that would admit of no fuzziness, but rested upon clear-cut, exact relations among figures, compelling absolute and unqualified assent. There were no reservations in the mind, nor could there be (so it seemed) in the world of space. Hold fast to this impregnable rock which no superior ingenuity can ever shake or render ambiguous in its outlines! Even science with its eye on the changing world could not resist the appropriation of this unsullied weapon.

The heretic appeared in no less a person than Newton himself, and his brilliant contemporary, Leibniz. Certain problems seemed exceedingly refractory to orthodox geometry, but proved capable of solution by a new method named by Newton the method of fluxions, but later known as the infinitesimal calculus. Essentially it implies that a formula which approaches a quantity nearer than any positively assignable error or defect does actually designate that quantity. The sum of an infinite series, $\frac{1}{2} + \frac{1}{4} + \frac{1}{8}$ + etc., although in any positive expression always differing from unity by the last named fraction in the series, is nevertheless equal to unity. Similarly the number nine repeated without limit after a decimal point equals one. In the long run the

intervening remainder becomes an "infinitesimal", which means in effect that it vanishes. This principle has become a very useful one in mathematics, but it did not escape unchallenged by the apostles of rigour. While such objections have been overridden, we can nevertheless understand the attitude of the conservative philosophers. They regarded themselves not merely as the exponents of a practical art, but as the custodians of the eternal verities which lay behind, and superior to, the world of appearance. If even minutest errors were allowed to pass the censor the fountain of truth was in danger of pollution at its source. We have something analogous today in the controversy over causality and indeterminacy. The conservative and other objectors to the calculus fell into the discard, and it is now generally held that an infinitely near approximation is indistinguishable from precision, at least in so far as that notion has any application to Nature. One might even go further and claim that exactness derives a somewhat negative meaning from the actual circumstances of its assignment, just as one cannot conceive infinity positively, but only as something to which no end can be assigned. Congruence of a measuring rod can at best be determined only within a margin of error, and so "absolute" congruence transcends possible experience.

In recent times orthodox geometry has undergone a change of role. Formerly it was the prosecutor, now it has become the defendant. Some hint of its ambiguous character was foreshadowed by Kant's schematism, in which space figured in a dual capacity, as at once ideal and real. It bridged the gap between the worlds of thought and objective Nature. But can space be allowed thus to masquerade under two diverse flags? Newtonian mechanics accepted the space of orthodox geometry as basic and indefeasible, but this tenet has been challenged by modern mathematics. Some at least of Euclid's axioms have been shown to be merely postulates, which are to be accepted or used only so far as they are found empirically to fit into Nature—other axioms and postulates being capable of substitution to produce new geometries which perhaps

provide a better frame for many facts of experience, and so supersede Euclid in that context. The great practical application of this principle was reserved for Einstein, who accepted the criticism that the boasted 'a priori', unimpeachable status of geometry was unfounded. Certainly it contained an ingredient of pure logic sharing the prerogative of reason itself; but it also contained an empirical element which was involved in all the vicissitudes and questionings of inductive science and could support its vogue only on pragmatic grounds.

Apart from the fact that I am not competent to trace the intricacies of the new geometries, it is undesirable that this brief attempt to record the evolution, and assess the philosophical significance, of the new attitude should be unduly encumbered by any attempt to do so. What I wish to emphasise is that this traditional and predominant engine of precision (orthodox geometry) has proved itself incompetent to frame completely the elusive manifold of Nature in hard lines of precision. We are all familiar with a point as that which has no parts and no magnitude; and we can realise how difficult it is to find such a thing in the objective world. Precision it may have, but, must we say, non-existence? Lines and superficies share the same fate. Dr. A. N. Whitehead has made a notable attempt to relieve geometry of the problem raised by points, lines etc. in their non-cubic phases as defined by Euclid. For a point Whitehead proposes a series of shells like the skins of an onion, the diminishing series to stand instead of the point. In a similar manner lines, surfaces, and even instants of time, are dealt with; and by thus taking conformations of positive figures instead of null abstractions he hoped, as he says, to relate the sensible with the sensible. No doubt this is a step towards realism, and it may provide an easement for some difficulties. Nevertheless, it may aptly be asked if perfect figures or boundaries can be found in Nature any more than points themselves.

It must be remembered that at least a much espoused view of the physical world regards it as being constituted and

conditioned (within the limits of possible experience) by our powers of measurement, or assessment by means of the senses (aided or unaided). If something is utterly beyond the reach of such senses it can scarcely be said to inhabit the world as we know it; and it is certain that the senses, even with the greatest conceivable aid from instruments, have some margin of vagueness. All the uncertainties and difficulties of exploration thus become real obstacles to the establishment of precise qualties or relations. Moreover, since the mathematician Minkowski, time has become inextricably interwoven with space, and it is doubtful if the ideal cross-section constituting simultaneity can be realised objectively. The world is flux permitting of no momentary pauses in which a perfect static figure can be described. We reflect again on the paradoxes of Zeno. A body moving from one place to another must traverse all the intermediate positions; yet if the body is in motion it cannot occupy a position for a finite time—that is, it cannot occupy it at all. This and other similar paradoxes have puzzled philosophers for two milleniums, and only in recent times has it been realised that the assignment of a static position to a moving body is not justifiable, and is responsible for the resulting paradox. The transfer of a precise ideal notion from the mind to Nature is the cause of the trouble. In other words, inappropriate first principles are applied. By using a different scheme of ideas, Frege and others have resolved the problem of motion without paradox. Yet I think it would be found that even these new principles, if applied without reservation, could lead to other paradoxes. Nature possesses a free and fuzzy character that will not be framed on exact lineaments.

A leading implication of the thoughts I am advancing is that precision, in so far as it has actual application at all, finds its proper domain in the intellect, and not in Nature. It would appear that man was at first hesitant to apply the perfection of his ideas to the outer world, then became bold and pushed them out towards the infinities with full confidence until the check of recent discovery revealed phenomena which

will not take the yoke so carefully prepared for them. Lord Kelvin spoke rather contemptuously of those who expressed diffidence in approaching alleged inconceivable numbers and magnitudes: "I leave inconceivability to the metaphysician" he declared, and then went on to speak of the "perfectly conceivable" multi-billions of the immensities. He had faith that his mechanical models could function in any amplification or diminution of scale. However, astronomy on the one hand, and sub-atomic physics on the other, have found sadly that Nature in these far off phases will not fit the old harness. Slight and unnoticed departures from precision in ordinary magnitudes have expanded into wide divergences when the remoter regions are explored; and so new weapons of thought must be forged. Nor is it to be supposed that these latter will prove competent to encage the whole universe in the meshes of precise thought. Such things as wave-atoms are well-known to hold the stage of present-day science, even if they are difficult to align with our orthodox and classical ideas.

It might be in order here to pass from shape and quantity to a consideration of number, which of all things might seem the type of unassailable precision; and one must admit that very difficult problems arise here. Is not Nature at least positive and clear-cut where numerical character is concerned? Two inches, two acres, two gallons, could never be isolated exactly; but surely two bricks are two indubitably and without any divergence however minute from just two. Nevertheless a difficulty arises here not very dissimilar from that affecting general terms, which cover up individual differences under a pseudo-uniformity. Number strictly should imply a repetition of absolutely similar entities; but Nature has no duplicates. Leibniz found his monads to have irreducible individuality, and a materialistic concept of similarity which ignores difference in time and space setting is out of keeping with modern physics. We may count "one, two, three", but, to whatever objects we apply the successive words, the objective series is not numerical purely and simply, but is polluted by irrelevant variations. Pure number is ideal, but it has

been vitally important in the method and history of science. Pythagoras initiated a great cult in acclaiming number as the essence of all things, and modern physics is close on his heels. Pure number is sought in atomic theories, but is elusive owing to physical vagaries, although atomicity, by which number is made to transcend quality, seems to promise the route to success. Two cities may be somewhat alike, two houses more so, and two bricks more amenable still to a single category. Push the analysis further and atoms are arrived at, by which it would appear that the world is being reduced to a uniformity. Character in the unit is disappearing. Atomic chemistry, however, still fell short of pure number, because different sorts of atoms had to be recognised. With the breaking up of the atom another step was taken towards entities less and less capable of characterisation by structure or property, but the total elimination of this has not yet been achieved. When it is brought about it would seem as if the Pythagorean world were approaching achievement. Meantime we must say that number itself cannot be transferred without some ambiguity from mind to Nature, or be found in Nature per se—at least in any Nature as yet envisaged by science. Yet there seems to be a perpetual seeking for unalloyed number. As an intermediary between Nature and intellect it seems to offer more promise than Kant's space and time which have now been vitiated from their pristine purity by relativity theory. Quanta of energy mark another advance towards atomicity in Nature—that is characterless number. As yet quanta, like the other fundamental particles, retain some differentiation and character—at least in their paths, and therefore in space-time specification; and it is difficult to suppose that complete similarity can ever be attained, with the consequent enthronement of pure number in Nature.

Some departure from strict logic in assessing Nature was recognised long ago, notably in the rise of inductive method as against the rationalism of the middle ages, but also in the logic of probability worked out by nineteenth century

mathematicians. An attitude fundamentally different however obtains today. In former times induction and probability were considered as more or less imperfect methods of *ascertaining* truth: the *fact* itself lay hidden in serene security, whether successfully brought to light (as was hoped) or not. Nowadays it is not a question merely of seeking to come by chance on a hidden treasure of truth to which no clear pathway leads, but the objective fact itself is not definite, and cannot in principle be absolutely defined. It is like exploring the South Pole; in old time its position was obscure by reason of the unsurmounted difficulties of the ice; but even after it is visited and subjected to most rigorous survey it will still be vague, because it is not in fact a definite point at all—wandering, as it does, over an area not quite definable.

Dr. Darwin's main argument was designed to show that ideal, rigorous thinking must give place to a wider mode of approach which does not despise induction, approximations, and probabilities. A notable contrast holds, he adds, between the way we think about things and the way we think we ought to think about them. Actually, we rarely conform to the ideals of Aristotelian logic, but lean upon arguments having no accurate axiomatic basis, which nevertheless compel belief because of some large accumulation of favourable evidence. I may venture as an illustration of my own the scooping of fish out of the water by means of a net: the holes of the net, far from being a weakness, are a source of strength as compared with a totally impermeable fabric. Yet that is not the point of the new probability: indeed it serves to emphasise an essential and easily missed distinction between the older outlook and the position of probability in the new physics. It is not that we use an imperfect weapon in order to succeed the better in capturing an elusive quarry, but that the quarry itself when in hand will not admit of precise and exclusive definition—any more than the end of the rainbow will admit of precise location. Darwin stated the application of these principles to causality as follows: "It is now easy to see that there was nothing wrong with the old inference that if

I know all about the present I can forecast the future exactly; the trouble was the impossibility of knowing the present." Such considerations reflect again on the dilemma of Zeno, who endeavoured to fasten precise location on moving bodies, with paradoxical results. In the case of the problems of gases statistical methods are not merely makeshifts helping out our inability to grapple with untold millions of errant particles: the real gas is itself the *whole* of the ensemble. Its essential character is incapable of attaching to isolated unit components. The moral of all this is, says Darwin, that the new physics has definitely shown that Nature has no sharp edges; and if there is a slight fuzziness attaching to all the facts of the world, then we must be wary if we attempt to draw a picture in hard outlines. "The inaccuracies and uncertainties of the world will be regarded in time to come as one of its essential features." He makes a strong plea for more emphasis in education on a logic of probability.

The apostle of rigour has not been silent under this new development. He recognises the case in which physics finds itself, and in some instances—Einstein and Planck, to quote two influential names—explicitly looks forward to the time when rigorous causality shall again come into its own. Meantime others are busy devising a mathematical technique capable of fastening vagueness itself in the shackles of formulæ, and thus developing as it were a precision of the second order. This is all to the good, but one may venture the prediction that all such systems will fail to impinge upon Nature with such fine needle points that no ambiguity remains. In truth all such attempts really amount to pushing Nature back into the region of thought. Darwin expresses the hope that some day a new synthesis of old and new theory may be made so that there will be one thing in the world that has not indefinite outlines—a new reformed principle of reasoning.

Are we then to conclude that in thought, and thought alone, precision can reside? I shall not pause to question the possibility of precision in thought. Suffice it that here, if anywhere, we must look for it to implement the modest

assumption that the whole idea is not a mere chimera. We are here faced with one more phase of the perennial antithesis between mind and matter: thought eternally projects itself on the objective world. It is doubtful if from the point of view of scientific progress it matters a great deal whether the worker is a realist or an idealist—a similar realm of phenomena confronts both. Nevertheless there is a danger, as history shows, in a science which supposes the outer world to be furnished with unshakable entities possessing a perpetual title to recognition and clearly defined domains, be these things entitled substances or laws. This does not exclude such more comprehensive ontology as our metaphysical attitude demands: but it leaves the investigating mind free to use all its resources, either in the form of intuitions or rationally elaborated theories, in investing Nature with such characters —mathematical or other—as best elucidate its behaviour. No law of Nature, unless it be in essence a tautology, is universally or completely true, as truth can be assessed by science. Such as appear infallible are not true empirical laws, but postulates of method; and even such postulates cannot be perfectly transferred from the originating mind to the actual presentations of the phenomenal events which confront our senses. Kant's famous tenet that synthetic propositions can be established "a priori" seems, as suggested already, to be stultified, if not annulled, by the discovery of Minkowski that space and time, the bridges by which he passed over from mind to objects, are now found to possess separable empirical and mental elements, not a homogeneous character participating in the attributes of both mind and matter. Intuitions there are, and must be; but, although the ambit of strict logic is shown to have limits, unchecked intuitions must not be allowed to run riot. This opens a large question reaching outside my present scope.

It is not to be supposed that the considerations advanced in this discussion are prejudicial to science—just the reverse. A full understanding of them can liberate science from conservative shackles and traditional bogeys. Sufficient approach

to precision and order can be obtained within the bounds of practical requirements, but never must the fallacy be allowed that extrapolation to endless limits or ultimate facts is necessarily, or even probably, justified. It is not a matter of a few curious paradoxes such as those of Zeno intruding in an otherwise perfect scheme, but of a (perhaps minute) germ of imperfection at the root of a whole system, the effect of which may be negligible except in the long run, but there possibly decisive and fatal. Man has often looked to Nature and natural law to find clues and prototypes of metaphysical things. Certainly suggestions may arise in contemplation of Nature leading the mind to some philosophical resting place; but no longer can such route claim special infallibility. Our world has seemed inviolably subjugated to the rules of classical geometry: to many of the ancients, as to Kant, it set a pattern of objective, if not of all, reality. Yet in cosmic astronomy and sub-atomic physics science has far outstripped Euclid and has applied mathematical method but remotely connected to the space and time of intuition. Moreover, these later methods themselves are but tentative, and a basic metaphysical pedestal must be constructed not upon any shifting ground of current scientific concepts, but upon intuitions, informed and controlled no doubt by rational instincts and the working entities of practical discourse, but in essence emerging freely from incomprehensible sources.

ARE PERCEPTS IN THE BRAIN?

By E. P. Edwards.

"For the most valuable is not he who leads his pupils to depend upon him, but he who leads those whom he instructs to be able to criticise their teacher." (Chapman Cohen.)

I.

There is no philosopher for whom I have a greater respect or from whom I have learnt more than Mr. Bertrand Russell. But not even the greatest of Mr. Russell's admirers would care to deny that his "Outline of Philosophy" is an exceptionally weak work, containing very much that is the result of confusion and very little that is of any value. What is most exasperating is that in this book Russell develops a theory of perception to which one is inclined to add Descartes's saying that "there is no theory or opinion, however absurd or incredible, which has not been maintained by someone or other of our philosophers". Those who had read Mr. Russell's previous book on "Our Knowledge of the External World" thought that this new theory—which combines all the faults of both what are called "causal" and what are called "representative" theories of perception—was a mere temporary lapse and that just as the representative theory contained in "The Problems of Philosophy" very soon gave way to the empirical and adequate account of "Our Knowledge of the External World", so this new lapse would soon be followed by a revised account. In this they were wrong, for in a book written eight years after the publication of the "Outline", Mr. Russell repeated the same theory almost word for word ("Religion and Science", pp. 128 ff). This theory of perception

would by no means be sufficiently important to warrant a whole article to refute it if it were not claimed (by its author) to be the direct and natural outcome of modern physics and if it were not the case that a number of well-known scientists hold very similar views, based also—so they tell us—on the results of physics. In showing the weaknesses of Russell's view I shall, hence, prepare for a more exhaustive *Critique of Physics*, in which it will be shown that most of the "scientific" theories that have so baffled the ordinary inquirer are the result of confused and careless thinking.

By "percepts" Russell in the "Outline" means more or less what he had previously called "sense-data"—"such things as colours, sounds, smells, hardnesses, roughnesses and so on".[1] A percept, in other words, is a datum to *one* sense. Thus when I now look at my table I see a percept of a certain colour having a certain shape, but one minute later when the sun is shining into the room I see a percept whose colour is different from that of the percept which I saw a minute ago. Also, when I move my body slightly to the left I see a percept whose shape is slightly different from the shape of either of the other two percepts.

Now, after knowing what is meant by a percept, one might think that one was in a position to reply to the question whether there were any such percepts in the brain or not. In the first place one would without hesitation reply in the negative as a consequence of the simple consideration that the brain was much too small to contain such big things as table- or chair-percepts. And in the second place, one would reply that the matter was open to empirical testing by looking into people's brains. We would then take someone who is looking at a table and describes to us the percept he is seeing and look into his brain to see whether or not we could find the percept there. If we could find it we would conclude that Russell's view that percepts are in the brain was correct, but if, despite careful search, we could not find it we should conclude that Russell was wrong.

[1] "Problems of Philosophy", p. 17.

Russell, however, would strongly object to such a naive approach to the question. Quite apart from the fact—he would rightly claim (for reasons to be discussed presently)—that the above gravely misrepresents his assertion about percepts being in the brain, the empirical test of looking into people's heads was irrelevant. For though it is natural, he admits, "to suppose that what the physiologist sees is in the brain he is observing", we must nevertheless not suppose that the physiologist's percept "is anywhere else but in the physiologist's head".[2]

II.

In order that we may not misrepresent Russell's view any longer we must proceed to outline it in greater detail and state it in a coherent and systematic manner. And first of all we must draw attention to Russell's distinction between the objects of perception and the objects of physics as well as between the spaces of perception and the one space of physics. The percepts (that is to say the objects of perception) as well as the perceptual spaces he regards as both private and mental,[3] and although the word "mental" is used in a somewhat unusual way (equivalent more or less to "in the physical brain") we may take it that the esse of both percepts and perceptual spaces consists in their percipi.

The existence of the objects and space of physics, on the other hand, does not consist in their percipi. And not only does their esse not consist in their percipi, but in fact no human being has ever perceived them or is ever likely to perceive them. We *infer* their existence for two reasons. The first of these two reasons is the similarity (or "sameness" as Russell inconsistently[4] puts it) of the percepts that are perceived by different observers. "The fact that it is possible for a number of people to perceive *the same* noise or the same

[2] "Outline of Philosophy" (from now on referred to as O.P.), p. 146.

[3] In "Problems of Philosophy" sense-data were said to be private but not mental.

[4] Russell has just before been arguing that percepts are private and mental, and that hence no two percipients ever perceive the same percept.

coloured pattern obviously depends upon the fact that a physical process can travel outward from a centre and retain certain of its characteristics unchanged, or very little changed." And again: "If there were not, in the physical world, processes spreading out from centres and retaining certain characters practically unchanged, it would be impossible for different percipients to perceive *the same* object from different points of view, and we should not have been able to discover that we all live in a common world."[5] The second of Mr. Russell's two reasons for believing in the existence, and mind-independent existence, of the objects of physics is more mundane—it is his constitutional incapacity for "believing that the sun would not exist on a day when he was everywhere hidden by clouds, or that the meat in a pie springs into existence at the moment when the pie is opened".[6] So, in order both to conciliate the Berkeleians and their arguments on the one hand and to retain the belief which his constitution dictates to him on the other, Russell admits that the perceptual sun and meat do not exist unperceived, but adds that the sun and meat of physics continue to exist and are in no way affected by the cognitions of human beings. In Russell's own words: "If the chair is to persist independently of being seen by us, it must be something other than the patch of colour we see, because[7] this is found to depend upon conditions extraneous to the chair, such as how the light falls, whether we are wearing blue spectacles, and so on. This forces the man of science to regard the 'real' chair as the cause (or an indispensable part of the cause) of our sensations when we see the chair."[8] To sum up what we have found to be Russell's view so far: what we immediately perceive is mental in one respect (and in our heads in another), but the percepts are not solely dependent on the percipient and afford us a ground for inference to their physical causes.

[5] O.P., p. 131 (my italics); cf. also p. 178: ". . . so that when we say they have all seen the same light we can only legitimately mean that their twelve stimuli had a common causal origin."

[6] O.P., p. 301.

[7] Here Russell conciliates the Berkeleians.

[8] O.P., p. 5.

The next question that Russell's theory of perception must naturally deal with is the question of what the physical causes of our percepts, what the objects of physics as contrasted with the objects of perception, are like. If, as Russell admits, we always only perceive percepts and never perceive the objects of physics, how can we then know what they are like? What evidence have we for believing, in fact, that to every percept or group of percepts *one* object of physics corresponds and for not believing that all percepts are caused by one and the same object of physics—that the physical universe consists of only one object? In replying to questions of this sort Russell admits that what we know about the objects of physics "is not their intrinsic character, but their structure and their mathematical laws". And in support of the contention that we *can* infer their structure, that we have a good reason for believing that our percepts of a brain and of meat respectively are caused by two different objects of physics and not by one and the same, he introduces the maxim 'same cause, same effect': "Their structure is inferred chiefly through the maxim 'same cause, same effect'. It follows from this maxim that if the effects are different, the causes must be different."[9] And to illustrate this statement, Russell adds elsewhere:[10] "When we hear a person speaking, the differences in what we hear correspond to differences in what he says; the effect of the intervening medium is roughly constant, and may therefore be more or less neglected. Similarly, when we see a patch of red and a patch of blue side by side, we have a right to assume some difference between the places from which the red and blue light come, though this difference cannot be supposed to resemble the difference between the sensation of red and the sensation of blue."

The last sentence quoted introduces Russell's view concerning the non-resemblance between percepts and the objects of physics. We must now inquire what this view asserts and the evidence Russell adduces in its support. "Physics is

[9] O.P., p. 163.

[10] "Religion and Science", p. 129.

mathematical", he writes, "not because we know so much about the physical world, but because we know so little: it is only its mathematical properties that we can discover. For the rest, our knowledge is negative."[11] "What is called a perception is only connected with its object through the laws of physics. Its relation to the object is causal and mathematical; *we cannot say whether or not it resembles the object in any intrinsic respect.*"[12] The reason for this view is twofold. Firstly, the objects of physics are (in themselves) in principle unobservable and we can therefore never know whether or not they possess qualities such as colours, sounds and so forth. Secondly, "The intervening medium always has *some* distorting effect: the red place may look red because of intervening mist, or the blue place blue because we are wearing coloured glasses."[13]

We have so far summarised and systematised a number of assertions that form part of Russell's theory of perception, but we have not yet expounded the evidence from which Russell thinks he can infer his conclusion that percepts are in the brain. There is some difficulty in stating this evidence since Russell's own account is unsystematic and confused,[14] but we must nevertheless now deal with it. There are, broadly speaking, two arguments which Russell here adduces, and though they are closely interconnected we may nevertheless treat them separately. The first we may call the argument from continuity, the second the argument from possible errors. The first is stated in connection with a description of what "really" happens when a physiologist observes another person's brain. "It is natural to suppose that what the physiologist sees is in the brain that he is observing. But if we are speaking of physical space, what the physiologist sees is in his own brain. It is in no sense in the brain he is observing, though it is in the percept of that brain, which

[11] O.P., pp. 163, 4.

[12] O.P., p. 155.

[13] "Religion and Science", p. 130.

[14] The fact that usually Russell's writings are a model of clarity and system and that here his account is so confused and obscure is also a sign that this part of his work is not his best.

occupies part of the physiologist's perceptual space. Causal continuity makes the matter perfectly evident: light waves travel from the brain that is being observed to the eye of the physiologist, at which they only arrive after an interval of time, which is finite though short. The physiologist sees what he is observing only after the light waves have reached his eye; therefore the event which constitutes his seeing comes at the end of a series of events which travel from the observed brain into the brain of the physiologist. We cannot, without a preposterous kind of discontinuity, suppose that the physiologist's percept, which comes at the end of this series, is anywhere else but in the physiologist's head."[15]

The argument from possible errors Russell regards as extremely important and in O.P. he emphasises it again and again. Put in general terms, the first part of the argument runs as follows: as the result of studying physics and physiology we know that "a given occurrence in the brain is capable of having a variety of causes, and where the cause is unusual common sense will be misled". To take an instance from the sense of sight we may say that it is theoretically possible "to stimulate the optic nerve in just the way in which light coming from the moon stimulates it; in this case, we should have the same experience as when we 'see the moon', but should be deceived as to its external source".[16] And similar illusions could be produced with other senses. From this point onwards the argument continues in some such way as this: What I can be most certain of in any given perception is a certain brain-event. As regards all the other elements—as the above instances show—I may be mistaken. Now, if I can see the moon though "there isn't any moon there" only because a certain brain-event occurs, then I must conclude that I do not really perceive the moon even "when she is there", but that I really perceive only the brain event and as the result of association or a conditioned reflex or mnemic effect or some other such mechanism (unconsciously?) infer

[15] O.P., p. 146.
[16] p. 173.

the moon's presence—as is clearly shown when my perception is illusory. And what is true of sight is also true of most, if not all, other senses.

From all of which Russell reaches the conclusion: "I know about what is happening in the brain exactly what naive realism thinks it knows about what is happening in the outside world."[17]

III.

In order that our criticism may be as clear-cut and as orderly as possible, we shall once more state Russell's views in a number of brief propositions. We shall number the conclusion-propositions with letters, and the evidence-propositions with numbers:

(A) The esse of percepts consists in their percipi.

(B) The existence of the objects of physics is known as the result of inference. Their existence is inferred (1) from the similarity of percepts, (2) in order to avoid idealism.

(C) The esse of the objects of physics does not consist in their percipi.

(D) The objects of physics are unobservable.

(E) Two or more (very) different percepts are caused by two or more objects of physics.
The chief evidence for (E) is (3) the maxim 'same cause, same effect'.

(F) There is no reason to suppose that the objects of physics are like percepts in certain important respects.

(G) From the standpoint of physics percepts are located in the brain. And the evidence for (G) consists of two arguments, one of which (4) we have called that from continuity and the other (5) that from possible errors.

Of the above propositions we regard (A) and (G) as false—or at least we shall argue that there is no reason

[17] p. 138.

whatsoever to believe them to be true, while we regard propositions (B) to (F) as pointless. It will not be necessary to discuss all propositions, outlined above, fully, since the discussion of some of them will be sufficient to show the pointlessness of the rest.

We may begin our criticisms by pointing out some self-contradictions. From (E) it follows that for any two different percepts we must postulate two different objects of physics as the respective causes. And yet our main basis for inferring the existence of the objects of physics is the assumption of one common cause for different, though very similar percepts. It may be said that the difficulty could be overcome by distinguishing between percepts which are very different and have separate causes and percepts which are rather similar and have a common cause. But quite apart from the obvious weakness and difficulties of such a distinction, Russell would not wish to assert it. His self-contradiction is (apparently, though of course only verbally) avoided—as we have seen in two quotations—by calling the very similar percepts 'the same'. But elsewhere, when he is out to establish (A) by means of Berkeleian arguments, he realises that these percepts are very similar, but not the same. We are thus driven to the conclusion that if (E) is true, (B) is not true, and that if (B) is true, neither (E) nor (A) can be true.

We must next try to find out whether the evidence adduced in support of (E) is actually sufficient to establish the proposition in question. Unless it can be shown that the maxim 'same cause, same effect' applies in this case there will be absolutely no reason to accept conclusion (E). And if, in fact, we ask how this maxim is established, what the evidence is from which it is inferred, we find that the evidence consists of numerous observed regular sequences *of percepts or groups of percepts,* and that the unobservable objects of physics never enter into it. According to the usual procedure adopted in inductive arguments, we would be entitled to assume that the maxim in question holds universally *within* the realm of percepts, but in order to be able to infer that it

also holds between percepts and unobservable entities like the objects of physics we should require some additional evidence. Yet no such evidence is forthcoming. We are thus driven to the conclusion that there is no evidence for believing that two different percepts are caused by two different objects of physics and that—for all we know—it may be the case that they are caused by the same object of physics or by an infinity of objects of physics. Thus when I am perceiving the percept of a star I have no more evidence for thinking that this percept is caused by the corresponding physical star than that it is caused by the physical moon or by Mr. Russell's physical hand or by any other object of physics or by any number of objects of physics. In other words, even if one could accept the view that there are such things as the unobservable objects of physics (a view which—as we shall see shortly—is absolutely without any factual basis), one would have to add that this world of the objects of physics is so utterly unknown that one could not tell whether there was one such object or two or any other number. The physical world would be reduced to an unknown X about which we would only know that it was the "remote cause" of our percepts. One wonders whether Russell would enjoy maintaining such a view.

We shall not in this paper criticise propositions (B) and (D) at full length since it is our intention to devote a separate paper to establish the more general thesis that (i) any proposition asserting the existence of an unobservable is a tautology and nothing more than a repetition of the observable evidence in different words, that (ii) any causal explanation in which the explanatory term is an unobservable can be shown to assert no more than the worthless proposition "x (being something observable) occurs because of that because of which it occurs", and that (iii) any inference from observable evidence to the existence of an unobservable is a tautology and no more than a restatement, in different language, of the observable evidence. It will at once be realised that (B) and (D) are among the propositions which our thesis would condemn as tautologies. Believing as we do, that this thesis is

sufficiently important to warrant a paper of its own, we shall here proceed to show how certain unjustifiable prejudices have led Russell to adopt such an impossible and quite unempirical view of perception. In showing Russell's analysis to be faulty we will begin by challenging proposition (A). But first we would note that in doing so we shall not deny any empirical facts, but only illegitimate conclusions drawn from them.

There are two senses in which, in O.P., percepts are said to be mental. The first sense (A) is the Berkeleian sense that their esse consists in their percipi; or, in different words, that they do not exist unperceived. The second sense is that physically they are to be located in the brain. It is with the first sense that we shall deal now. For Russell's assertion that percepts are mental (in the first sense) there appear to be two sorts of reason. The first is never explicitly put and is no more than a suspicion on our part, but we believe that every careful reader of Russell's writings would share our suspicion. The suspicion in question is that Russell—like the majority of writers on psychology—seems to regard perceptions (owing, no doubt, to the implicit reasoning that the perception is the final event of a series whose last physical event occurs in the brain) as "little mental things" like, for instance, images; and while they differ from images in many respects they do not differ from them as regards their "mentality". Finding later through introspection that no such event as perceiving can be discovered apart from the object perceived, Russell comes to identify percept with perception, concluding that percepts, too, are mental things. The second sort of reasons, adduced in support of (A), are entirely Berkeleian in character.[18] Taking three such reasons as typical, we get (i) the laws of perspective, (ii) "how the light falls", (iii) "whether we are wearing blue spectacles". Let us consider (i). The usual reasoning in this connection might be put as follows: (a) X, which is a physical object (here in the naive sense), is observed to have different lengths (figures, shapes)

[18] Cf. A. J. Ayer, "The Foundations of Empirical Knowledge", pp. 1-11, for a clear exposition of similar arguments.

from different points of view at the same time T_1. (b) It is evident that nothing can have more than one length (figure, shape) at the same time T_1. And from these two propositions it is concluded that none of the many observed lengths is "real" and that each depends for its esse on percipi. In reply to the above argument one may adopt a number of different courses of which the most convenient one would be to deny the truth of (b). We do, of course, admit that the facts described in (a)—in a convenient, but somewhat inaccurate manner—could be described in such a way that (b) would not be false, but, as they are put, (b) cannot be true if (a) is true since (a) simply shows that there are things which have more than one length. As long as we use the language of (a) to describe the facts in question we shall, if we want to be consistent, have to say that things do have more than one length. We must insist here that the Berkeleian conclusion depends precisely on the apparent contradiction of (a) and (b) and that as soon as the ambiguity is cleared up we realise that the argument has no bearing on the question whether percepts are mental or not.

It may be profitable to insert at this stage some remarks concerning the ways philosophers (and particularly writers on perception) are misled into the most absurd discussions and conclusions as the consequence of (for the purpose of practical life justifiable) inaccuracies in our ordinary speech. Let us, as an example, take the case when 99% of mankind "see a thing X"[19] as red while 1% see X as green. In ordinary life we proceed to say that X is "really" red and if anyone tells us that X is green we say that he is wrong. Then philosophers come and rightly find that in the ontological sense of "real" X is "really" green as much as it is red, but then instead of simply stating the empirical facts they continue to bring in propositions like (b) above and conclude that X is neither red nor green, becoming either "critical realists" of some sort or Berkeleians. Others again confuse

[19] We do not assert that there is a thing X apart from the percepts, but use this expression only as a brief way of conveying what we mean.

the use of "real" as opposed to imaginary, with "real" as used for specially favoured (or "deified", to use Mr. D. Taylor's expression) percepts and conclude first that the less favoured percepts are imaginary and therefore mental; later they realise that ontologically the status of deified and non-deified percepts is not different and conclude that all percepts are mental. The chief source of confusion consists in the fact that the philosophers concerned do not see that "real" is a summarising name and not a separate empirical quality or property like "green" or "red". With this point, however, we shall deal more fully later on when we will also suggest the best possible language whereby to avoid pointless pseudo-conflicts. Here we may rather quote a brief passage from Russell to show how words like "real" or "same" (the latter is another constant source of confusion) have deluded them. "When my boy was three years old", he writes, "I showed him Jupiter, and told him that Jupiter was larger than the earth. He insisted that I must be speaking of some other Jupiter, because, as he patiently explained, the one he was seeing was obviously quite small. After some efforts, I had to give it up and leave him unconvinced."[20] Hardly a better example of the general confusion we have been pointing out above could be found. Russell's theory of the external world applied to Jupiter would run somewhat along the following lines: the real size of Jupiter is much larger than that of the earth; nevertheless all these Jupiterian percepts I apprehend are rather small; hence I conclude that the real size of Jupiter does not belong to any Jupiterian percepts, but to the Jupiter of physics whose existence I infer.[21] We can now easily see that this nonsense could have been easily avoided if Russell had realised the only intelligible meaning of "real size" in this context, namely, that if certain possible percepts were

[20] O.P., p. 136.

[21] Following on the above quotation Russell writes (p. 136): "In the case of the heavenly bodies adults have got used to the idea that what is really there can only be inferred from what they see; but where rats in mazes are concerned, they still tend to think that they are seeing what is happening in the physical world. The difference, however, is only one of degree, and naive realism is as untenable in the one case as in the other."

actualised—ourselves putting measuring rods round Jupiter, etc.—we should find that this planet was larger than the earth. Russell's little son was also quite right regarding the fact that his percept did not confirm his father's assertion: in the strict sense of "different" they were each referring to a different Jupiter, namely the little Russell to his actual percept at the moment, and the old Russell to certain possible percepts. But there is also a (less stringent and more confusing) sense of "same" and "different" in which they were referring to the same Jupiter. Cutting away, then, the nonsensical remarks about an unobservable physical Jupiter, they were not disagreeing about the empirical facts, but only using alternative languages to describe them.[22]

Returning to our discussion of (A) we have to note that, quite apart from the fact that the various relativistic arguments do not entail the conclusions which Russell and others have derived from them, we cannot accept the proposition in question since no possible evidence could ever establish it. This unverifiability of (A) Russell has himself seen at one stage when he tells us that "we cannot find out what the world looks like from a place where there is nobody, because if we go to look there will be somebody there; the attempt is as hopeless as trying to jump on one's own shadow".[23] And yet precisely such a jumping on one's own shadow would be required to verify the assertion that percepts do not exist unperceived. However, in rejecting (A) we have no intention of supporting any of the realists who assert that the esse of percepts or of physical objects stands in no dependent relation to their percipi. On the contrary we believe that Berkeley, though he went slightly too far, thus landing himself in unverifiable assertions, was on the right line as against realists who believe in propositions like (C) above. We have already pointed out that (in our opinion) propositions asserting the existence of such unperceivable objects can be shown to be hidden tautologies.

[22] Cf. Ayer, op. cit., pp. 46 ff.
[23] O.P., p. 164.

We can now, at last, approach proposition (G) according to which percepts are located in the physical brain. In rejecting this view we shall advance, in addition to the criticisms that have already been brought forward and bear indirectly upon (G), two distinct kinds of arguments. Firstly, we shall point out a fatal self-contradiction by looking at the evidence for the evidence for (G). Secondly, we shall contend that the evidence-propositions (4) and (5) afford no good grounds for inferring (G).

First, then, let us examine the evidence for the evidence for (G). Russell's chief ground for locating percepts in the physical brain is, as we have seen, the fact that the last physical, or, rather, externally observable, event in the series that precedes a given perception occurs in the brain. Now, under no circumstances could Russell have drawn (G) as conclusion from this fact unless the brain observed in such cases was the physical brain (or one of the innumerable actual and possible percepts which the brain *is*). And similarly with other items of the evidence. The nerves, the sense-organs, the scientific instruments in question would—in order to give (G) the least shadow of plausibility—have to be physical nerves, physical sense-organs, physical instruments. Their esse, in other words, unlike that of percepts, could not consist in their percipi. However, if (G) is true then all the items of the evidence—as percepts—are in the brain of the percipient, depend for their percipi on their esse. The brain, nerves, instruments are not physical objects, then, but percepts. Russell's mistake here was his overlooking the fact that in "establishing" his conclusion, in establishing a conclusion which (as Russell himself frequently points out) is a flagrant contradiction of the view that percepts are "external" and of the view that a physical thing simply is a collection of actual and possible percepts, of the view that, in other words, we are, in perception, directly acquainted with physical things—in adducing the evidence for this proposition he was presupposing the truth of precisely those propositions whose truth (G) contradicts. The self-contradiction is of the following nature:

if (G) is true then the evidence for (G) is untenable and hence (G) is untenable.

Our second criticism of (G), or rather group of criticisms, is of much greater importance than the first, though it is not logically conclusive like it. It is so much more important because we must here uncover the whole mistaken approach to perception which is the ultimate cause of such absurd theories as Russell's and of those of many other "critical" realists. To consider this mistake which, as we shall show presently, is the source of so many absurdities in epistemology we can hardly do better than look at the following passage: "When we 'perceive' the sun, there has been a long causal process, first in the ninety-three million miles of intervening space, then in the eye, the optic nerve and the brain. The final 'mental' event which we call seeing the sun cannot be supposed to bear much resemblance to the sun itself."[24] By "seeing the sun" Russell here simply means the percept, the yellow patch which we perceive when looking at the sun. We have already above objected to percepts being regarded as little mental things. Here we must both elaborate this objection and point out that it is just this step from the account of the physical process to regarding the percept as a mental event (as in the above quotation) which causes the absurd conclusion of solipsism or representative and causal theories of perception. In challenging this step we do not in any way challenge the observed facts—the physical process preceding perception. We only say that the inference that percepts are mental is the result of an unjustifiable prejudice and that there is no evidence whatever for this view. In denying that percepts are mental, however, we have no intention of asserting that they are "physical" in the sense in which "physical" means "existing unperceived"; this latter view we think as baseless as the former. Nevertheless we regard percepts as physical—in the only intelligible sense which we can assign to this word.

[24] "Religion and Science", p. 128.

There are some who might say that in spite of the above considerations percepts are mental (in the sense in which images are mental) since A's percepts, for instance, unlike his teeth or his nose or his hair, cannot be perceived or observed by others and are in this respect like images. But this is clearly untrue. We can perceive A's percepts or, at least, percepts exceedingly like his and we can state the conditions under which we would perceive the same percepts. That is to say, if B occupies a spatial perspective very close to A's and if his sense-organs etc. are rather like A's then he will perceive very similar percepts; and from many data we can inductively infer—with as good or as bad a right as we do in any high-scale-probability induction—that if anyone could ever again occupy exactly A's spatial perspective (which of course will never in practice happen) then he would perceive the same percepts. This should suffice to answer the above objection and to show that percepts are perceivable by outsiders in a sense in which images are not and that it is illegitimate to say that percepts are "in the mind" (or brain) in the sense in which images, for instance, can be said to be.

It has been mentioned before that Russell's fatal step from the physical evidence to the "mentality" of the percepts is bound up with a certain prejudice which is, alas, only too common among writers on perception—both philosophers and psychologists. We must now look a little more closely at this prejudice which may aptly be called the "localisation-prejudice". Russell's view on this head and the reasons he gives have been stated so many times in this article that we need not repeat them; and we may thus begin our criticism at once. In our exposition of the evidence for (G) we have seen that Russell locates percepts in the mind on the one hand and in the physical brain on the other because of the brain-event's being the last in the publicly observable series and because the possibility of error is least concerning the brain-event. The real and more mundane reason for locating percepts in such a manner appears to be that Russell, like most people, regards the mind as a sort of receptable bordering

on the brain. Any events, then, which follow brain-events and have a traditionally mental flavour about them are put into this receptacle. And since in his physicalistic moments Russell identifies the mind with the physical brain they are also located in the brain. But, whatever the reason or reasons be which induce a person to hold such a view, its groundlessness is plain to anyone who realises that the evidence is quite irrelevant to the conclusion. Suppose that I am looking at a cigarette which is lying on the floor. The percept which I see in such a case is located by me outside myself or my brain—on the floor. But, says Russell, the physical cigarette and the percepts of it are not located in the same place. To this we may reply that firstly we have already given reasons and shall give others why we cannot believe that there are physical cigarettes which are not actual or possible percepts and that, hence, we shall never get to the mysterious non-percept cigarette at all; and secondly—as we have seen in our investigation of the evidence for the evidence—unless our percepts in the case of our observations of sense-organs, brains etc. are regarded as the physical things then (G) is self-contradictory. But if they are so regarded we must admit that percepts are located outside our brain. In addition, the fact that our percepts are invariably preceded (or accompanied?) by brain-events of a certain sort or the fact that different stimuli may precede the same percept—neither of these facts has any bearing on the issue. They must simply be noted as interesting contingent facts.

For the sake of clarity it would be useful to make clear firstly in what sense percepts are "external" when they are said to be localised outside the brain and secondly to restate Russell's evidence-propositions in such a way that, so far from causing unwarranted conclusions, they will be seen to describe certain important (and by some probably unsuspected) empirical facts. In attempting this restatement we shall also probably arrive at the most convenient description of the facts—of what really happens. What happens when I see a cigarette lying on the floor is that a white patch of a certain

length is perceived (by me) and is localised—or perceived as localised—outside my body and lying on the floor. But when this percept is said to be outside or external to myself this must not be understood to mean that it exists unperceived.[25] "External" here only stands for certain empirical properties to distinguish the percept from images, for instance, which do not share these properties. "Externality" is a summarising name, but not a separate property. Among the empirical properties summed up by this name we could find (i) that percepts are not the effects of our volitions, (ii) that in our case the visual percepts are correlated—i.e. they are the basis for inferences to possible percepts of other senses—with future tactile and olfactory percepts, (iii) that the percepts are permanently possible to certain spatial perspectives or that the occupants of neighbouring perspectives perceive very similar percepts—that percepts are in this sense public; and other similar properties.

In order to restate Russell's evidence-propositions in a way that is not only not misleading but even helps us to realise an important fact, we will consider the case when A perceives a percept X—a yellow patch, we shall assume; and the seeing of this yellow patch would normally be described as "seeing the moon"—and localises this percept far away, outside of his brain. From various other investigations and observations we also know that whenever A perceives X this is immediately preceded (or accompanied?) by a certain brain-event or brain-condition which we shall call Y. We will further assume that A himself possesses all this information about the Y-X relationship. Now, the two important empirical facts which Russell's "argument from possible errors" helps us to realise might be stated in the following manner: (a) Given A's knowledge about the Y-X relationship, he can, on perceiving X, infer to Y with greater confidence than he can to any other percept which is usually associated with X. That is to say: it is hardly possible that A's inference to Y should

[25] Though if by "existing unperceived" no more is meant than "(permanently) possible to a certain perspective" we may say that it exists unperceived.

turn out to be incorrect, but it may quite well occur that A's inference to X_{1-10}—certain other visual percepts—and to Z_{1-10}—certain tactile percepts—will turn out to be wrong —when for instance A discovers on leaving the house that he "really" saw a lighted advertisement on the roof of an opposite building. (b) A percept X[26] may be a member of very different collections or groups or families of percepts (when we here speak of a percept as member of a group we mean no more than that it is frequently associated with certain other percepts and that its presence affords a basis to infer to some, or all, of the others). And this it may be in two ways. Firstly, as in our instance with the moon, X may be associated with X_{1-10} and with Z_{1-10} (the moon) or it may be associated with rather different visual and tactile percepts (the lighted advertisement). Secondly, it may occur in different series as regards sense-organs and nerves. That is to say, on perceiving X, A can with enormous confidence infer to Y, but he cannot with equal confidence infer to the preceding events in or conditions of his sense-organs or of the relevant nerves—for, as Russell points out "the effect in the brain is what is necessary to your sensation . . ., and by suitable experiments this sensation can be made quite deceptive".[27] A may know that in most cases when he perceived X he could correctly infer to sense-organ condition p and to nerve-condition q—at the relevant times outsiders could have perceived p and q respectively. Now, Russell's argument from possible errors goes to show that while in most cases X occurs in the series p-q-Y-X, it may on some occasions occur in the series Y-X where p and q are totally absent or even in the series m-n-Y-X.

To illustrate the above re-statement we may consider an instance which is very much like those that are usually offered as the basis for inferring that percepts are in the mind or in the brain. My pencil is dropped on the floor and the noise I

[26] You may say it is "the same" percept or that there are two (or more) different percepts, but the quarrel will not be factual; it will only be between alternative languages to describe the same facts.

[27] "Let the People Think", p. 112.

hear is very much like that (it isn't really, but we shall assume that it is; it would not be difficult to find an example where we need not make this assumption) which I hear when it is thundering in the distance. What does this example teach us? What can we conclude from it? Certainly not that the auditory percept(s) are in my brain. We may conclude, or rather simply say what the empirical facts are, namely, that very similar (or the same) percept(s) occur in rather different contexts or groups or collections.

Summing up this discussion, we may say that if by the assertion "percepts are in the brain" no more is meant than that every percept is preceded (or accompanied?) by a certain brain-event then we do not object to it, except that it is likely to cause confusions because of the usual sense in which the words are employed. But if by (G) more is meant—and Russell means more—then we must reject it as a baseless assertion and add that the evidence in its support leads us to see certain important facts, but does not warrant (G). And to apply to those who point this out dyslogistic terms, to say that they "are condemned to a view of perception which is miraculous"[28] is merely to betray once again one's prejudice that because the antecedent brain-event is in the brain the consequent percept must also be "inside". If our own view is to be called "miraculous" we should have to call every empirical fact "miraculous". Moreover, if anyone has a right to speak of miracles here it is ourselves when we draw attention to the fact that it is only by miraculously absurd reasoning that Russell ever gets outside his "physical brain", just as those who assert percepts to be in the mind get outside their minds only by a series of fallacies. Just as the latter are logically condemned to solipsism so, too, is Russell—though it is a solipsism of the brain. It is not necessary to elaborate this point since any critical student of Locke will be familiar with such an elaboration. There is also no need to add anything in connection with the mysterious sun, stars, tables etc.

[28] O.P., p. 147.

of physics which Russell postulates in order to escape solipsism.

IV.

It remains for us to learn certain general lessons and draw certain general conclusions on the whole subject of perception from this rather lengthy refutation of Russell's theory of perception. As a preliminary we want to note our agreement with both Mr. Ayer and Mr. G. A. Paul that rival accounts of perceptions—such as for instance Dawes Hicks's on the one hand and Alexander's on the other—are not rival hypotheses in the way in which rival scientific theories are rival hypotheses. They do not lead their authors to have different expectations, nor is there any disagreement about the occurrence of the facts which these theories set out to describe. They are rather alternative languages—the differences are (i) verbal, (ii) in feelings (?), (iii) in images.[29] It might be thought that for people who, like Paul, Ayer and ourselves, approach perception in such a way the subject loses all interest. To believe this, however, would be to be mistaken. There remain still three things to be done for those who hold views like ours. Firstly, because the quarrelling factions do not realise the verbal character of their quarrels and further because they frequently find themselves in a cul-de-sac they are apt to make empirical assertions which are definitely untrue. Our first task then would be to point out the falsehood of such assertions—for instance that since the act of perceiving is mental, the percept must be mental; that "the three elements involved in the perception of an object are the act of thought, the content of the act, and the object . . .";[30] that though our percepts are mental or semi-mental they afford us a reason for inferring to a mind-independent external world; etc., etc. Secondly, we must show how the various quasi-factual rival accounts could arise—how it is that the "epistemologists" like Meinong, Alexander, and many others

[29] Cf. John Wisdom, "Other Minds" (II); *Mind*, January, 1941, pp. 8 ff.
[30] C. E. M. Joad: "Introduction to Modern Philosophy", p. 14.

got away from the empirical facts into fictitious theories and disputes. Thirdly, we must see what language it would be most convenient to use in describing the facts if we are not satisfied with the ordinary language employed because of its inaccuracy and its likelihood to mislead the unwary. The latter task has recently been attempted by Mr. Ayer,[31] but for reasons that cannot here be enumerated at length we regard his language as even more misleading than that of common-sense.

We cannot in this article cover the whole field which an exhaustive treatment of these three tasks would demand, but we shall prefer to discuss representative instances instead. In connection with the first task we shall say a few words about what is called "inference (usually unconscious!) in perception". This latter assertion that we never directly perceive the external thing, but infer to it on the basis of what we directly perceive is one which is often met with in writings on perception. It is common to practically all causal and representative theories and it is quite natural that those who are first taken in by the relativistic arguments and then wish to escape solipsism should employ some such prop as this. Judging from my personal experience, however, I have to state that I have never come across such acts of inference. Nor have those—so they admit when they are pressed—who would write whole chapters, arguing that such inferences takes place. The inference, at this stage, becomes unconscious. At this stage, too, anyone who is not emotionally attached to the pro-inference view will have realised that to say "the inference is an unconscious one" means about as much as "I admit that there is no evidence for supposing that such an inference occurs, but unless it did occur my beautiful theory would break down and I should become the prey of endless worries". But although we may justly say that it is astonishing that any sober person should ever have failed to realise what we may call the "immediacy" of perception and while we may also

[31] In "Foundations of Empirical Knowledge".

justly condemn as silly Russell's view when he writes[32] "he (Watson) fails to realise that almost as long and difficult an inference is required to give us knowledge of the rat's bodily movements as to give us knowledge of its 'mind' ", there is *some* factual basis for this inference-view. For it cannot be denied that inference of a certain sort accompanies most of our perceptions, even if it consists in nothing else than a certain bodily tension or the fact that we feel surprised if this inference turns out to be false. But this inference is not from percepts to unperceivable objects, but from present percepts to future, or, quite generally, to possible, percepts. Never realising the last-mentioned point, the representative and causal theorists then proceed only too eagerly to offer this genuine inference as the empirical basis for their view, though (as has been pointed out) they prefer, since it is safer, to speak of unconscious inferences.

One of the chief sources of confusions in books on perception is the fact that so many philosophers regard words like "external", "objective", "real", etc. as standing for genuine empirical qualities in the way in which words like "red" or "green" or "hard" stand for empirical qualities. We have already made the point that "external" is a summarising name and not an empirical quality. Regarding "objective" and "real" there is an additional source of confusion in the fact that these words are loosely used in different senses and that this is frequently not realised. Let us for a moment return to the example given above, where 99% of mankind saw a thing X as red while 1% saw X as green. If now a philosopher asks: "But what is X's objective colour?" we must point out to him that he may be asking one of three things. He may be asking (i) what X's colour is when X is unperceived, or (ii) what X's colour is to the majority (to the 99%) or (iii) what X's colour is to more than one perceiver. In sense (iii) X's colour is both red and green, in sense (ii) X's colour is red, but as regards sense (i) we cannot give a reply. Some philosophers, indeed, who have not realised that the word

[32] O.P., p. 135.

"objective" is used in the above three senses do give a reply to (i)—they unwittingly deify the majority in some such way as this: D.H.[33]—whom we suppose to be such a philosopher—is asked by A what the objective colour of X is without specifying the sense in which he uses the word "objective". D.H. takes it to be sense (ii) and replies: "red". Next, B comes along and asks D.H. what X's objective colour is and points out that he uses "objective" in sense (i). D.H., who never was quite aware that "objective" is also used in other senses, immediately replies: "Funny, you know, A was just here and asked me the same question, so I can reply to your question at once. X's objective colour is red."

A moment ago, we said that we cannot reply to (i). Some philosophers may thereupon have dissolved into a mournful masochism over our irremediable ignorance. As a sort of drug we might quote to them a sentence that occurs in the works of Sigmund Freud:[34] "The problem of the nature of the world irrespective of our perceptive apparatus is an empty abstraction without practical interest." Some philosophers might object to our exposition and say that if God were to perceive X he would perceive X's objective colour in the sense that He is perceiving that colour which exists though no human being perceives it. With these philosophers we have no quarrel, but we must point out to them that our ignorance regarding (i) cannot be remedied until God passes this information on to us and that our view on unobservables makes us gravely doubt whether He ever will. The God-hypothesis in this case, in other words, we regard as empty and worthless.

We have already shown one way in which the use of "real" misleads philosophers. In what follows we will deal with this matter in greater detail; indeed, we regard what follows as the chief thesis which we propound in this article. First of all we shall try to give an empirical account of the facts without introducing "real" and similar words in any

[33] D.H. is not a fictitious philosopher. For a good passage of this sort see G. Dawes Hicks, "Berkeley", pp. 122, 3.

[34] "The Future of an Illusion", pp. 97, 8.

misleading fashion. After that we shall illustrate our assertion regarding the misleading character of "real" with a closely examined instance.

Percepts are described as "real" in two different contexts. Firstly, they are contrasted with the "unreal" or "illusory" images. Secondly, some percepts are contrasted as "real" or "veridical" with others which are alternatively described as "unreal" or "false" or "non-veridical". In order to avoid confusions it is necessary to keep these two uses apart. Here we are primarily concerned with the second usage, but we must preface the discussion of this with a short remark on the first contrast. It is not difficult to draw a very sharp line[35] between percepts and images, and while it may not be possible to say in the case of some extremely rare (and for the most part hypothetical) data whether they are images or percepts we can do this in the enormous majority of actual cases. Having made this remark, we may now proceed to our description of the facts in question. We can divide percepts into two classes: (A) Those of which we can truthfully assert the following two properties: (a) that they, or percepts very much like them, are perceived by the (vast) majority of percipients, and (b) that they can be correlated, according to specified conditions, with percepts that are perceived through other senses. (B) Those percepts of which we *cannot* truthfully assert (a), or (b), or of which we cannot assert either (a) or (b). To illustrate by an example: a stick is leaning against a wall and I describe my percept as being (a) red in colour and (b) a straight line (or a narrow rectangle) in figure; I ask other people occupying neighbouring perspectives for the colour of their percepts and they tell me that theirs is also red in colour; I then approach the stick and find that my tactile percepts are such as I usually describe as those of a straight line. I hence consider my original percept to belong to class (A). Another man sees the "same" stick as straight, certainly, but as green in colour. His percept belongs to (B) as do those of all people when they see a stick immersed in water. The extreme case of (B) is the percept of a stick

[35] Cf. Russell, "The Analysis of Mind", pp. 179 ff.

immersed in water whose colour is green while to the majority of people it is red. *I submit as my thesis that if philosophers had in the past confined themselves to such a description as the one just sketched or to one which in essential features is like it, none of the so-called philosophical "problems" of perception would ever have arisen.*

Where, then, must we seek the source of these fictitious problems? The answer is: in the fact that when philosophers came to call percepts of the class (A) "real" or "veridical" and percepts of the class (B) "unreal" or "false" or "non-veridical" they failed to see that in calling a percept "real" they were simply asserting its possession of (a) and (b)[36] as well as giving it a new name, but that they were not asserting any additional property. The "reality" of a percept is simply a new name and not some new property over and above (a) and (b)—perhaps "emerging" out of them. To put our criticism in different words: percepts of the class (A) are not real, they are *called* "real"; those who perceive percepts of the class (B) are not wrong, they are *called* "wrong", etc. The confusion becomes particularly bad when the question is asked "are these percepts real" or "which percepts are real", as if such questions were like the question "are these percepts green". The questions which, for the sake of clarity, one should ask instead are the questions: "are these percepts called real" and "which percepts are called real" or one may leave the old form and add (or at least imply): "but when we ask whether these percepts are real or not we are not looking for a quality like greenness or hardness, but simply ask whether or not these percepts possess properties (a) and (b)". Provided that "real" etc. are used, with an addendum of this sort implied, an account of the facts may be given in which they are used and which is yet not misleading.[37] It may here

[36] (a) and (b) are by no means the only properties which determine whether a percept belongs to class (A) or to class (B). There are, as we shall see, others as well such as a definite spatial distance from the percipient's body and definite media. (a) and (b) have been selected only for purposes of exposition—so that our account can be briefer and more lucid.

[37] This is what in substance Ayer (op. cit.) does. But because of the traditional associations of words like "real" and the endless elucidations

be added that in the ontological sense of "real" and "unreal" all percepts are either "real" or "unreal"—they are all percepts.

In order further to illustrate the contrast between our empirical description and the confusions which occur in other accounts we select one of Berkeley's arguments. Philonous-Berkeley questions whether secondary qualities—here colours—inhere in physical objects. "Are the beautiful red and purple we see on yonder cloud really in them?"[38] If the red and purple inhere in the cloud how are we to explain that 'its colour' would be blackish if we sailed through it in a balloon? Which is its real colour? Again, no object retains its ordinary colour when observed through a microscope and if it be asserted that the real colour is that seen through the microscope the question may be asked: how powerful must a microscope be so that we can see the real colour? And if a definite answer be given to this question we may conclude that none of the colours seen through any other (more or less powerful) microscope will be real (which conclusion—it is pointed out by Berkeley—is obviously absurd). Again the same object has a different colour from different spatial standpoints; and even from a given point of view it appears different by artificial light from what it appears in sunlight. Lastly, to people with differently constituted perceptual apparatus (e.g. colour-blind people) the same object will have more than one colour. We have seen how Berkeley's idealism is inferred from such facts as we have just enumerated. Since we saw at the same time how easy it is to show that Berkeley's conclusion is not implied by the evidence we need not repeat this criticism at the present stage. Instead we shall first give an empirical description of the facts in our non-misleading way and then make some comments on the views of those who are misled. And first we would draw attention to what we have already said in footnote 36: that (a) and (b) are not the only properties which determine our putting percepts into (A) or (B). As the

involved when they are used in a non-misleading fashion we believe that our account is preferable.

[38] "Three Dialogues", (Everyman Edition), p. 214.

above instances show there are others as well; for instance, (c) a more or less definite spatial distance between the percept and the percipient's body; (d) a medium of a certain sort[39] (e.g. normal or fairly normal sight, but not perception through microscopes).

After this preliminary remark, then, let us give our empirical account of the facts. First of all, we must point out that all the data enumerated in the Berkeleian example are equally percepts—have ontologically the same status. This fact, indeed, is realised both by Berkeley and the critical realists, though it is not realised by unreflected common sense or by majority-deifiers à la Dawes Hicks. Amongst these percepts we find that a selected number possess properties (a), (b), (c), and (d) and we hence put them into class (A). (It should be noted that "being in class (A)" is not an additional property over and above possessing (a), (b), (c) and (d), but merely a convenient way of expressing these empirical properties in a brief way.) In our case it is the red and purple percepts that are put in class (A). All the other percepts lack one or more of the properties that would qualify them for (A). The percepts which we would perceive if we sailed through the cloud in a balloon lack property (c)—the usual or normal spatial distance. None of the percepts seen through the microscope can be put into (A) since they all lack (d)—the normal medium—and the same can be said about the percepts seen by artificial light. Lastly, the percepts seen by colour-blind people lack property (a).

We have more than once pointed out that percepts of the class (A) are *called* "real" in ordinary life, while percepts of the class (B) are *called* "unreal". When the place of "calling" in this matter is forgotten or—as is the case with most epistemologists—never noticed, the confusions begin. They all begin to ask: "but which of these percepts are real?" and the more naive ones amongst them, by kicking stones and by otherwise playing on the emotions of their audience and themselves, conclude that the percepts which in ordinary life are called

[39] The distances and media selected are, as Ayer rightly points out, those having the greatest predictive value.

"real" really are real. The "profounder" ones amongst them say: no percept of the class (B) is real, but there is no qualitative difference between percepts of classes (B) and (A). Hence all percepts are unreal—and hence mental or semi-mental or God knows what. The latter philosophers consist of the Berkeleians and the critical realists. Philosophers of both the naive and profound types have sufficiently been dealt with in this article so that all that remains to do is to quote Dawes Hicks's "reply" to Berkeley. In the light of the preceding pages our commentary should be so obvious as not to require explicit statement: "If normal vision be the way in which the real colours of things are more or less accurately apprehended, is it not a strange demand to make that abnormal vision must also likewise be a way of more or less accurately apprehending them?"

It has frequently been contended that such facts as the Zöllner or Müller-Lyer optical "illusions" lead to a Berkeleian or critical realist conclusion. In order to show once again the language we advocate in describing such facts—when we perceive what "isn't really there"—we will briefly say what happens in the case of such "illusions". We see percepts which we find not to correlate (as other "real" percepts do) with lots of other visual and tactile percepts. The former, in other words, lack property (b) and are hence put into class (B). And this is all that there is to it.

V.

The upshot of this article is that as long as we stick to the empirical facts, as long as we simply and clearly describe them no difficulties or "problems" arise. But where "problems" do arise which could never be, even in theory, solved by the methods of science we may conclude that the problems are fictitious and the result of a failure to guard against one or more misleadingly used words. This is true, in our opinion, not only about the subject of perception, but about all philosophical "problems".[40]

[40] For a not dissimilar view of the determinism-freewill "controversy" see Moritz Schlick, "Fragen der Ethik", pp. 105-116.

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